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Turnout and Representation in Presidential Primary Elections

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Decision Making in Soviet Cities

Robert J. Lieber

Interest Groups and Political Integration: British Entry Into Europe

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APSA Presidential Address: To Nurture a Discipline

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CONTENTS

11 Where the Votes Come From: An Analysis of Electoral Coalitions, 1952–1968 Robert Axelrod

21 Turnout and Representation in Presidential Primary Elections Austin Ranney

38 Decision Making in Soviet Cities B. Michael Frolic

53 Interest Groups and Political Integration: British Entry Into Europe Robert J. Lieber

68 Political Clientelism and Ethnicity in Tropical Africa: Competing Solidarities in Nation-Building René Lemarchand

91 Patron-Client Politics and Political Change in Southeast Asia James C. Scott

14 On Anarchism and the Real World: William Godwin and Radical England Isaac Kramnick

29 Congress and the Defense Budget: 1960–1970 Arnold Kanter

44 A Theory of Political Ambition: Career Choices and the Role of Structural Incentives Gordon S. Black

60 The Committee Function: An Influence Equation Thomas W. Casstevens

64 APSA Presidential Address: To Nurture a Discipline Robert E. Lane

83 Communications

88 Editorial Comment

91 Book Reviews

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ARTICLES

11 **Where the Votes Come From: An Analysis of Electoral Coalitions, 1952–1968.** This paper describes how the contributions that different groups make to a party's total strength can be specified by breaking down each contribution into its three components—size, turnout, and loyalty. Through the use of survey data on reported vote rather than party identification, the voting coalitions of the Democrats and Republicans are analyzed. By means of examples of selected groups, the actual magnitude of these contributions and their components are presented for each of the last five presidential elections. Major attention is given to a group-by-group evaluation of the generally accepted view that the Democrats are a coalition of diverse overlapping minorities including the poor, Blacks, union members, Catholics and Jews, Southerners, city dwellers and perhaps the young; and the view that the Republicans have appeal for the corresponding non-minorities. The empirical results show which aspects of these views are valid and which are not. Some implications for the party system as well as the strategic considerations inherent in this approach are also discussed.

By Robert Axelrod, Assistant Professor of Political Science, University of California, Berkeley.

21 **Turnout and Representation in Presidential Primary Elections.** This paper provides a partial test of the widely-held assumption that preference primaries are the most representative element of the presidential nominating system. It notes that the average voting turnout in presidential primaries is only 39 per cent, compared with 69 per cent in the same states' ensuing general elections. The representative quality of these electorates is examined with data from sample surveys of the 1968 New Hampshire and Wisconsin primaries. A comparison of the primary participants with nonparticipants among each party's identifiers shows, first, that presidential primary electorates are demographically unrepresentative of their nonvoting fellow partisans in age, education, income, and social status. Second, primary participants have no more intense party identifications than do nonparticipants. Third, participants hold issue positions more strongly than nonparticipants do, and on some issues they even hold positions contrary to those of nonparticipants. Finally, both of the Democratic primaries overrepresented Johnson sentiment and underrepresented McCarthy and Kennedy sentiment. The author concludes that the preference primaries' claim to be the most representative element of the presidential nominating system may not be warranted, especially if and when the national conventions are reformed along lines such as those laid down by the McGovern-Fraser Commission. In any case, 1972 offers a unique opportunity to study the comparative representativeness of local, state, and national party conventions and the preference primaries.

By Austin Ranney, Professor of Political Science, The University of Wisconsin, Madison

38 **Decision Making in Soviet Cities.** How are decisions made in Soviet cities? Who are the municipal decision makers? What kinds of decisions do they make? Is there a Soviet urban political system? This article attempts to answer these questions by focusing on four aspects of decision making in Soviet cities: budget formulation, the planning process, housing construction and allocation, and the staffing of key municipal posts.

Urban autonomy has increased in the past decade, but Soviet municipalities are very much restricted in their decision making. Superior Party and governmental authorities continue to dominate the decision-making process and any decision made by municipal authorities can be vetoed by superior Party and governmental organs.

Soviet municipal decision making is now being influenced by three tendencies: municipal administrators are acquiring more influence in municipal government and administration; the educational qualifications and professional expertise of city Party members are rising; *ad hoc* citizen and group interest articulation may be developing. Comparisons between Soviet and North American urban decision-making models are useful and valid, although they require an improved methodology and much more Soviet data.

By B. Michael Frolic, Associate Professor of Political Science, York University.

53 **Interest Groups and Political Integration: British Entry into Europe.** This article analyzes the influence of interest groups in the formation of British policy toward participation in European unity From the important theoretical literature bearing on the subject, it derives and tests two hypotheses relating group behavior to the political integration process. The hypothesis imputed to the group politics approach holds that if interest groups influence policy formation, then progress toward

integration is likely to be impeded. The hypothesis suggested by an extension of the functionalist approach implies that if interest groups influence policy formation, then progress toward integration is likely to be facilitated. The data indicate a confirmation of the first hypothesis and a rejection of the second. Groups were unfavorable toward European unity for both organizational and economic reasons. They restrained Britain's movement toward participation in integrated European ventures until the onset of effective politicization. Then the introduction of broadly conceived national interest considerations displaced cost-benefit calculations as the criteria for judgment and diminished the groups' influence. At least in the task of enlarging a geographic area of integration, if not in expanding that integration once a grouping already exists, a conscious political decision was found to be essential.

By ROBERT J. LIEBER, Assistant Professor of Political Science, University of California, Davis.

68 **Political Clientelism and Ethnicity in Tropical Africa: Competing Solidarities in Nation-Building.** The concept of political clientelism is one if the few genuinely crosscultural concepts available to political scientists for the comparative study of transitional systems. As a descriptive concept, political clientelism helps us uncover patterns of relationships which deviate markedly from those ordinarily associated with class or ethnicity. As an analytic concept political clientelism provides crucial insights into the internal dynamics of social and political change. Moreover, if, as some contend, patterns of resource allocation are more meaningful indicators of political development than their conceptual opposites, political clientelism may well supply the critical "missing link" between micro- and macro-sociological or system-centered theories of political development.

By RENÉ LEMARCHAND, Professor of Political Science, University of Florida.

91 **Patron-Client Politics and Political Change in Southeast Asia.** The analysis presented here is an effort to elaborate the patron-client model of association, developed largely by anthropologists, and to demonstrate its applicability to political action in Southeast Asia. Inasmuch as patron-client structures are not unique to Southeast Asia but are much in evidence, particularly in Latin America, in Africa, and in less developed portions of Europe, the analysis may possibly have more general value for understanding politics in preindustrial societies. After defining the nature of patron-client ties and distinguishing them from other social ties, the paper discriminates among patron-client ties to establish the most important dimensions of variation, examines both the survival and transformations in patron-client links in Southeast Asia since colonialism and the impact of major social changes such as the growth of markets, the expanded role of the state, and the creation of local regimes. Finally, the paper shows how patron-client bonds interact with electoral politics to create distributive pressures which, in turn, often lead to inflationary fiscal policies and vulnerability of regimes to losses of revenue.

By JAMES C. SCOTT, Associate Professor of Political Science, University of Wisconsin.

114 **On Anarchism and the Real World: William Godwin and Radical England.** Much that is characteristic of contemporary anarchist thought can be found in the writings of the founder of that tradition, William Godwin. He is ambivalent on the value of technology and modernity, nostalgic at one moment and progressive another. He extols individual autonomy while preaching community solidarity. Above all he shares with modern anarchism an elitist disdain for ordinary men and women, which in the case of Godwin leads to an unresolved tension between theoretical radicalism and practical conservatism. His anarchist doctrine repudiates all forms of coercion, law, and government. It eschews cooperation of all kind as deleterious to individual development. At the same time it posits an ideal order characterized by a high degree of informal coercion practiced by zealous neighborly inspection. But Godwin was a less than eager friend of reform and agitation during the years of Pitt's repression of radical movements in England. This was clear even in the pages of *Political Justice,* but all the more obvious in his feud in 1795–96 with John Thelwall and the London Corresponding Society, the leading radical activists. This paper outlines Godwin's anarchism and points out the implications of his dispute with Thelwall. In addition it shows the extent to which Godwin has given anarchism certain of its enduring qualities.

By ISAAC KRAMNICK, Associate Professor of Political Science, Yale University.

129 **Congress and the Defense Budget: 1960–1970.** It is commonly held that Congress has made only inconsequential changes in the President's requests for defense appropriations. Scholarly studies of

the budgetary process, notably the work of Fenno and Wildavsky, do not include defense spending in their analyses. For nondefense spending they find that Congress has primarily a *fiscal* rather than *programmatic* orientation toward appropriations.

House and Senate changes in the President's defense appropriations requests were analyzed for the fiscal years 1960 through 1970. It was found that small Congressional changes in total defense spending obscured more substantial activity in particular budget categories. The concentration of Congressional activity in two of these categories—Procurement and R&D—indicates that Congress has had a significant programmatic orientation toward defense spending. Changes in the level of Congressional activity across time confirm this hypothesis.

Efforts to measure the relative influence of the House and Senate proved inconclusive. The Senate dominated the conference committee, but the Senate's influence was concentrated in the least important budget categories. The House made most of the changes in the President's budget requests, but very few of such changes were appealed by the Defense Department to the Senate.

By Arnold Kanter, Assistant Professor of Political Science, The Ohio State University.

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144 **A Theory of Political Ambition: Career Choices and the Role of Structural Incentives.** Officeholders periodically face the problem of choosing among a set of career alternatives, and these alternatives customarily include the choice of dropping out of political life, or seeking reelection, or of choosing to seek higher office. This paper assumes that officeholders behave according to a rational calculus in making such choices, and that the main elements involved in the choice process include the probabilities and values attached by the candidate to his alternatives, and the investments required to obtain these alternatives. Political ambition, or the desire to seek higher office, is shown to develop as a product of the investments that politicians make in their political careers, and the investments are shown to be associated with the structural characteristics of community size and electoral competitiveness. The subjects of the research are 435 city councilmen from 89 cities of the San Francisco Bay Region, and the data include information derived from interviews with the councilmen and aggregate election data collected on each city.

By Gordon S. Black, Assistant Professor of Political Science, University of Rochester.

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160 **The Committee Function: An Influence Equation.** In legislative systems a bill is commonly considered and reported by an appropriate committee before it is considered on the floor of the house. Since motions on the floor frequently relate to such bills, it is often apposite to refer to a motion's relevant committee. This article presents a mathematical model of the relationship between the relevant committee's divisions on the floor and a motion's probability of passing. Let x be the proportion of the relevant committee voting yea on the floor, z be the proportion of the relevant committee voting nay on the floor, and y be the proportion of the relevant committee neither voting yea nor voting nay on the floor, then a motion's probability of passing is $(x^2 + y^2)/(x^2 + y^2 + z^2)$. The fit between theory and observation is quite good: six hundred eleven roll calls from the 90th and 91st Congresses have been analyzed; 0.628 of the motions actually passed; and 0.613 of the motions were expected to pass.

By Thomas W. Casstevens, Associate Professor of Political Science, Oakland University.

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164 **APSA Presidential Address: To Nurture a Discipline.** Discipline based associations have gone through two stages: "learned society" and "professional association." Because of the inadequacies of the market for ideas and talent, it is time for a third stage or model, "science management." Certain characteristics of the discipline affect the utility of this model, including the level of training (relatively modest), the resources available to the discipline (lower than other social sciences), and disciplinary morale and cohesion (relatively high).

There has been an increase in doctoral programs in political science at the rate of about three new programs per year for the past twelve years. Many of these new programs are of doubtful quality, but they may be useful to the discipline in terms of teacher training (although less useful for research training), if their doctorates replace persons with the master's degree, and if they add to, rather than substitute for, a full complement of persons trained by the older quality programs. Regionalization of doctoral programs is opposed, and some characteristics of quality programs are stated: relatively large faculties and student enrollments, and high proportions of research oriented full professors. Discipline wide enrollment policies should be from "the top down" in terms of quality of the programs.

Creative political science can be identified and encouraged by attention to its constituent elements, conditions, and the selection of students identified by measures of creative potential. Creative research productivity is more related to motivation than to intelligence (beyond a certain level); a productive research career is directly and strongly related to the quality of a person's graduate program and the quality of his employing institution, as well as to the eminence of his dissertation sponsor. Individual and collective research productivity has increased; the consequent information explosion requires more systematic information exchange and a policy of encouraging the most fruitful research agendas for the discipline.

By Robert E. Lane, Professor of Political Science, Yale University.

3 **COMMUNICATIONS**

From Gerald H. Kramer, Theodore S. Arrington, David Nexon, W. Scott Thompson, Douglas W. Rae and Michael Taylor, Kenneth A. Shepsle.

8 **EDITORIAL COMMENT**

BOOK REVIEWS

1 **Political Theory, History of Political Thought and Methodology**

Black, *Monarchy and Community: Political Ideas in the Later Conciliar Controversy, 1430–1450.* Paul E. Sigmund, p. 191
Hobbes, *A Dialogue Between a Philosopher and a Student of the Common Laws of England* (edited by Joseph Cropsey). J. Roland Pennock, p. 192
Jorrín and Martz, *Latin American Political Thought and Ideology.* Edward J. Williams, p. 193
Kruskal, ed., *Mathematical Sciences and Social Sciences.* Oliver Benson, p. 194

5 **American Government and Politics**

Adams, *The Growing Church Lobby in Washington.* R. Wallace Brewster, p. 195
Beck, ed., *Law and Justice: Essays in Honor of Robert S. Rankin.* John R. Schmidhauser, p. 196
Bollens, Bayes and Utter, *American County Government: With an Annotated Bibliography.* Samuel K. Gove, p. 197
Burton, *Democrats of Oregon: The Pattern of Minority Politics 1900–1956.* Lester G. Seligman, p. 198
Domhoff, *The Higher Circles: The Governing Class in America.* Earl Latham, p. 199
Feingold, *The Politics of Rescue: The Roosevelt Administration and the Holocaust, 1938–1945.* Morton J. Tenzer, p. 201
Fitch and Walsh, eds., *Agenda for a City: Issues Confronting New York.* Andrew Hacker, p. 203
Haveman and Margolis, eds., *Public Expenditures and Policy Analysis.* Walter G. Held, p. 204
Hinckley, *The Seniority System in Congress.* George Goodwin, Jr., p. 205
James, *The Years of MacArthur. Vol. 1: 1880–1941.* Dale Pontius, p. 206
Kurland, ed., *Felix Frankfurter on the Supreme Court: Extrajudicial Essays on the Court and Constitution.* D. S. Hobbs, p. 207
Lindbergh, *The Wartime Journals of Charles A. Lindbergh.* Alexander DeConde, p. 208
Marcus, *Grand Old Party: Political Structure in the Gilded Age, 1880–1896;* and Yearley, *The Money Machines: The Breakdown and Reform of Governmental and Party Finance in the North, 1860–1920.* Hugh Douglas Price, p. 210
Merelman, *Political Socialization and Educational Climates: A Study of Two School Districts.* Pauline Marie Vaillancourt, p. 211
Navasky, *Kennedy Justice.* Nelson W. Polsby, p. 212
Nimmo and Oden, *The Texas Political System;* and Anderson, Murray and Farley, *Texas Politics: An Introduction.* Joseph L. Bernd, p. 214
Ries, *Executives in the American Political System.* Lloyd A. Rowe, p. 215
Russett, *What Price Vigilance? The Burdens of National Defense.* Kenneth E. Boulding, p. 217
Tyler, *The Political Imperative: The Corporate Character of Unions* and *The Labor Revolution.* Melvin A. Kahn, p. 218
Wasby, *The Impact of the United States Supreme Court: Some Perspectives.* Twiley W. Barker, p. 220
Wright, *Federal Grants-in-Aid: Perspectives and Alternatives.* Parris N. Glendening, p. 221

2 **Comparative Government and Politics**

Allen and Johnson, eds., *African Perspectives: Papers in the History, Politics, and Economics of Africa Presented to Thomas Hodgkin.* Sheldon Gellar, p. 222
Anderson, *The Nordic Council: A Study of Scandinavian Regionalism.* Joseph B. Board, p. 223
Backer, *Priming the German Economy.* F. A. Breier, p. 225
Chao, *Agricultural Production in Communist China 1949–1965.* Ramon H. Myers, p. 226

Cohn, *Economic Development in the Soviet Union.* A. Nove, p. 227

Crowder and Ikime, *West African Chiefs: Their Changing Status Under Colonial Rule and Independence.* Lloyd A. Fallers, p. 228

de Robien, *The Diary of a Diplomat in Russia, 1917–1918;* Kenez, *Civil War in South Russia, 1918;* and Silverlight, *The Victors' Dilemma: Allied Intervention in the Russian Civil War, 1917–1920.* Robert F. Byrnes, p. 229

Gamarnikow, *Economic Reforms in Eastern Europe.* Robert Owen Freedman, p. 231

Gann and Duignan, eds., *Colonialism in Africa 1870–1960.* Volume Two: *The History and Politics of Colonialism 1914–1960.* Harvey Glickman, p. 232

Gordon, *Self-Determination and History in the Third World.* Rupert Emerson, p. 233

Green, *The Containment of Latin America: A History of the Myths and Realities of the Good Neighbor Policy;* and Duncan and Goodsell, eds., *The Quest for Change in Latin America: Sources for a Twentieth Century Analysis.* Alexander Edelmann, p. 235

Groennings, Kelley and Leiserson, eds., *The Study of Coalition Behavior: Theoretical Perspectives and Cases from Four Continents.* Steven J. Brams, p. 236

Havens and Flinn, eds., *Internal Colonialism and Structural Change in Colombia,* Reid R. Reading, p. 238

Hazard, *Communists and Their Law: A Search for the Common Core of the Legal Systems of the Marxian Socialist States.* Harold J. Berman, p. 240

Hill, *God's Englishman: Oliver Cromwell and the English Revolution;* and Breen, *The Character of the Good Ruler: A Study of Puritan Political Ideas in New England, 1630–1730.* Michael Walzer, p. 241

Hilliker, *The Politics of Reform in Peru: The Aprista and Other Mass Parties of Latin America.* Edward C. Epstein, p. 242

Johnson, *The Cameroon Federation: Political Integration in a Fragmentary Society.* Claude E. Welch, Jr., p. 243

Journalist M., *A Year is Eight Months;* and Sviták, *The Czechoslovak Experiment, 1968–1969.* Joseph Hajda, p. 244

Kochan, ed., *The Jews in Soviet Russia Since 1917.* John A. Armstrong, p. 246

Kornberg and Musolf, eds., *Legislatures in Developmental Perspective.* Hans H. Baerwald, p. 248

Kumar, *Essays on Gandhian Politics; The Rowlatt Satyagraha of 1919.* Joan V. Bondurant, p. 249

Leach and Mukherjee, eds., *Elites in South Asia.* Theodore P. Wright, Jr., p. 251

Levinson and Brightman, eds., *Venceremos Brigade: Young Americans Sharing the Life and Work of Revolutionary Cuba;* and Reckord, *¿Does Fidel Eat More Than Your Father?: Conversations in Cuba.* Patricia W. Fagen, p. 252

Loomis and Rytina, *Marxist Theory and Indian Communism.* John Patrick Haithcox, p. 254

Malloy, *Bolivia: The Uncompleted Revolution.* Robert D. Tomasek, p. 255

Mboya, *The Challenge of Nationhood: A Collection of Speeches and Writings.* Brack Brown, p. 257

Moine, *Résultats des élections belges entre 1847 et 1914.* Val R. Lorwin, p. 258

Myrdal and Kessle, *China: The Revolution Continued;* and Hsiung, *Ideology and Practice: Evolution of Chinese Communism.* Pi-chao Chen, p. 259

Nagy-Talavera, *The Green Shirts and the Others: A History of Fascism in Hungary and Rumania.* Randolph L. Braham, p. 261

Petrov, *Mongolia: A Profile.* Paul Hyer, p. 262

Powell, *Social Fragmentation and Political Hostility: An Austrian Case Study.* Robert D. Putnam, p. 263

Rhodes, *The Government of London: The Struggle for Reform.* Wallace S. Sayre, p. 264

Rivkin, *Nation-Building in Africa: Problems and Prospects* (edited by John H. Morrow). I. William Zartman, p. 266

Rotberg and Clague, *Haiti: The Politics of Squalor.* Jean-Claude Garcia-Zamor, p. 266

Ruin, ed., *Scandinavian Political Studies, Volume 5/1970.* Dankwart A. Rustow, p. 267

Sharp, *The Ministry of Housing and Local Government.* Fred Tickner, p. 269

Spencer, ed., *Religion and Change in Contemporary Asia.* Donald E. Smith, p. 270

Suh, *Documents of Korean Communism 1918–1948.* John Kie-Chiang Oh, p. 272

Tuden and Plotnicov, eds., *Social Stratification in Africa.* P. C. Lloyd, p. 273

Tyrmand, ed., *Kultura Essays.* S. L. Andreski, p. 274

Ugalde, *Power and Conflict in a Mexican Community: A Study of Political Integration.* Carolyn Needleman and Martin L. Needleman, p. 274

Walter, *Student Politics in Argentina: The University Reform and Its Effects, 1918–1964.* Peter G. Snow, p. 276

277 International Politics, Law and Organization

Beck, Deutsch, Raup, Rose and Turnbull, *The Changing Structure of Europe: Economic, Social and Political Trends.* Stuart A. Scheingold, p. 277

Butler, *The Soviet Union and the Law of the Sea.* Kazimierz Grzybowski, p. 278

Corbett, *The Growth of World Law;* and Franck, *The Structure of Impartiality: Examining the Riddle of One Law in a Fragmented World.* Wesley L. Gould, p. 279

George, Hall and Simons, *The Limits of Coercive Diplomacy: Laos, Cuba, Vietnam.* George Modelski, p. 280

Gupta, *The Fulcrum of Asia: Relations Among China, India, Pakistan, and the U.S.S.R.;* and Simmonds, *China's World: The Foreign Policy of a Developing State.* Loren W. Fessler, p. 282
Heath, *Old World, New Horizons: Britain, Europe, and the Atlantic Alliance;* and Calleo, *The Atlantic Fantasy: The U.S., NATO, and Europe.* Francis A. Beer, p. 283
Jervis, *The Logic of Images in International Relations.* Paul Kecskemeti, p. 285
Ogley, *The Theory and Practice of Neutrality in the Twentieth Century.* Urs Schwarz, p. 286
Šveics, *Small Nation Survival: Political Defense in Unequal Conflicts.* Carsten Holbraad, p. 287
Thorp, *The Reality of Foreign Aid.* Doris A. Graber, p. 288
Toscano, *Designs in Diplomacy: Pages from European Diplomatic History in the Twentieth Century* (edited by George A. Carbone). Norman Kogan, p. 289
Tung, *China and the Foreign Powers: The Impact of and Reaction to Unequal Treaties.* Samuel Shih-Tsai Chen, p. 290
Vâli, *Bridge Across the Bosporus: The Foreign Policy of Turkey.* Carl M. Dibble, p. 291
Van Ness, *Revolution and Chinese Foreign Policy: Peking's Support for Wars of National Liberation.* Shao-chuan Leng, p. 293

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Where the Votes Come From: An Analysis of Electoral Coalitions, 1952–1968*

ROBERT AXELROD
University of California, Berkeley

Where do the Democrats get their votes rom? Where do the Republicans get *their* votes rom? Because of the great interest of scholars, oliticians, and the public in the changing shape f electoral politics in America, these questions ave been asked over and over. V. O. Key has rgued that one party or the other usually dominates American politics for decades by putting ogether a stable winning coalition and that ince the Depression the dominant party has been the Democrats.[1] He has, however, also pointed to the existence of slow but steady ecular trends in electoral coalitions.[2] In 1968 he rise of a third party challenge and the election of a Republican President led to numerous reassessments of the future of the party coalitions. Kevin Phillips's assertion of *The Emerging Republican Majority*[3] is perhaps the best known of these recent prognostications, but a critical review by Nelson Polsby[4] has already undermined much of Phillips's argument. The Republican Ripon Society has given a liberal nterpretation of the *Lessons of Victory*,[5] while he less partisan interpretation of Scammon and Wattenberg has emphasized the size of the middle groups in American politics.[6] The long term cyclical pattern in American electoral politics has been reported by Sellers,[7] and Burnham[8] and Converse *et al.*[9] have done excellent studies of some of the factors in recent presidential elections. Yet, a number of basic questions about the sources of partisan support remain.

Surprisingly enough, even professional politicians have only a rough and ready idea of how their own national coalitions are formed. For example, many do not know whether Blacks contribute more or less than union members to the vote totals of the Democratic party. As another example, we may ask whether the common wisdom is really correct that the Republicans do worse than the Democrats among the young and the poor. Questions such as these require measurement of groups and their voting behavior. Measuring where the parties have been and where they are now is the first step toward a sophisticated analysis of where they are going.

This paper has two purposes. The first is analytic—to specify the components of a group's contribution to a party's electoral coalition. The second is empirical—to measure the actual magnitudes of the contributions that have been made by selected groups in each of the last five presidential elections.

The Meaning of a Contribution to a Coalition

What does it mean to ask how large a contribution a group makes to the electoral total of a party? Clearly, a large group can contribute more votes to a party than can a small group. It is also true that a group with a high turnout can contribute more votes than a slightly larger group with a lower turnout. And yet it is also true that a relatively small group with a poor turnout can contribute votes to a party quite out of proportion to its size if the group loyally gives overwhelming support to that party. Thus measurement of the contribution of a group to a party's total vote must depend on three factors: the size of the group, its turnout, and its loyalty to the given party.

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[1] V. O. Key, "A Theory of Critical Elections," *Journal of Politics*, 17 (February, 1955), 3–18.

[2] V. O. Key, "Secular Realignment and the Party System," *Journal of Politics*, 21 (May, 1959), 198–210.

[3] Kevin Phillips, *The Emerging Republican Majority* (New Rochelle, New York: Arlington House, 1969).

[4] Nelson W. Polsby, "An Emerging Republican Majority? Review Essay," *The Public Interest*, No. 17 (Fall, 1969), 118–126.

[5] Ripon Society, *The Lessons of Victory* (New York: Dial Press, 1969).

[6] Richard M. Scammon and Ben J. Wattenberg, *The Real Majority* (New York: Coward-McCann, 1970).

[7] Charles Sellers, "The Equilibrium Cycle in Two-Party Politics," *Public Opinion Quarterly*, 29 (Spring, 1965), 16–38.

[8] Walter Dean Burnham, "American Voting Behavior and the 1964 Election," *Midwest Journal of Political Science*, 12 (February, 1968), 1–40. See also his *Critical Elections* (New York: Norton, 1970).

[9] Phillip E. Converse, Warren E. Miller, Jerrold G. Rusk, and Arthur C. Wolfe, "Continuity and Change in American Politics: Parties and Issues in the 1968 Election," *American Political Science Review*, 53 (December, 1969), 1083–1105.

Each of these three components of a group's contribution is important. Much confusion arises in political discussions about how the parties build their coalitions because no one of these components tells the whole story. Only when all three components are taken into account can the actual contribution of a group be fully determined.

The coalition literature from game theory is of little help here because of its assumption that groups are unified actors. Each group is assumed to be able to turn out all of its members and deliver them with complete loyalty to the coalition of its choice.[10] While this assumption is suitable for certain legislative bodies,[11] it is a gross distortion when applied to a mass electorate whose turnout and loyalty are always less than complete.

Fortunately, the three components of size, turnout, and loyalty can be fitted together in a rather simple way to determine a group's contribution to a party's vote total. The formula is:

$$\text{Contribution} = \frac{(\text{Size}) \times (\text{Turnout}) \times (\text{Loyalty})}{(\text{National Turnout}) \times (\text{National Loyalty})}$$

This formula says that if you multiply the size, turnout, and loyalty of a given group, and divide the result by the product of the national turnout and national loyalty, you get the magnitude of the contribution of that group to the electoral coalition of the party.[12] The *size* of a group is the proportion of all adults of voting age who are members of that group. The *turnout* of a group is the proportion that voted in a given election.[13] The *loyalty* of a group to certain party is simply the proportion of th votes of that group which are cast for tha party. The *national turnout* is the proportion o all adults who vote, and *national loyalty* is th proportion of all votes that go to the give party. Finally, the *contribution* of a group t the electoral coalition of a party equals the proportion of all the party's votes that come from members of that group.

The value of this formula is that it specifie how the three components can be combined t form the total contribution of a given group. I allows us to study contributions of a group ove time, to determine how the contribution is attained in terms of the separate components and to compare the contribution of one grou to that of another.

Since any combination of groups may b chosen for analysis, the groups will typicall overlap. A comparison can still be made between the overlapping groups in terms of thei separate contributions, size, turnout and loyalty, but these factors cannot be added together directly. For example, in 1968 the poor (i.e., families with incomes less than $3000 per year) contributed 12 per cent of the Democratic votes, while the Blacks contributed 19 per cent. The contribution of the group consisting of those who are poor or Black (or both) can be determined by adding the separate contributions and subtracting the contribution of the overlapping group, namely the poor Blacks. Since the poor Blacks contributed 5 per cent of the Democratic votes, the contribution of those who were poor or Black (or both) was 12% + 19% − 5% = 26%. A similar procedure applies to measuring the *size* of the group which is the union of two overlapping groups.

The procedure for handling overlaps readily generalizes to any group describable in terms of more than two component groups.[14] This means that the contribution and size of any

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[10] See John von Neumann and Oskar Morgenstern, *Theory of Games and Economic Behavior*, Second ed. (Princeton: Princeton University Press, 1947), and R. Duncan Luce and Howard Raiffa, *Games and Decisions* (New York: Wiley, 1957).

[11] See William H. Riker, *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962), and Sven Groennings, E. W. Kelley, and Michael Leiserson, *The Study of Political Coalitions* (New York: Holt, Rinehart and Winston, 1970).

[12] The proof involves nothing more complicated than substituting the definitions of terms for their names and cancelling whenever possible.

[13] In calculating turnout, one counts all adults of voting age, whether or not they were eligible to vote.

[14] John Kemeny, J. Laurie Snell, and Gerald Thompson, *Introduction to Finite Mathematics*, Second ed. (Englewood Cliffs, N.J.: Prentice-Hall, 1966), pp. 116–122.

$$\frac{(\text{size})\,(\text{turnout})\,(\text{loyalty})}{(\text{natl. turnout})(\text{natl. loyalty})} = \frac{\left(\dfrac{\text{no. in group}}{\text{no. in nation}}\right)\left(\dfrac{\text{voters in group}}{\text{number in group}}\right)\left(\dfrac{\text{voters for party in group}}{\text{voters in group}}\right)}{\left(\dfrac{\text{voters in nation}}{\text{number in nation}}\right)\left(\dfrac{\text{voters for party}}{\text{voters in nation}}\right)}$$

$$= \frac{\text{voters for party in group}}{\text{voters for party}}$$

$$= \text{contribution.}$$

group can be analyzed if the relevant information is available about the component groups and their intersections. While this type of information is difficult to get from aggregate data, it is readily available from survey data. Thus, the poor, the Blacks, the poor Blacks and those who are poor or Black can all be analyzed if desired. This paper will fully report a selected set of overlapping groups and note a few of the interesting properties of their intersections and unions.

It should be clear that the term "contribution" as it is being used in this study refers to the proportion of a party's total votes that is provided by a given group. It does not tell how the votes were gathered or even whether the group membership provides a reference point for its members. For example, it does not measure a group's contribution in terms of money, policy, or organizational strength. It simply counts what ultimately matters in elections, namely votes.[15]

The Coalition Members

Since the New Deal there has been a generally accepted answer to the question of where the Democrats get their votes. The answer is that the Democrats are a coalition of diverse overlapping minorities: the poor, Blacks, union members, Catholics and Jews, Southerners, and city dwellers. In the last few years it has been suggested that, at least for presidential elections, the Southerners may have left the Democratic coalition while the young people have entered it.

The Republicans are a different matter. It is possible to think of the Republicans also as a coalition of minorities such as the rich and the people of Anglo-Saxon descent. But the Republican party itself tries to avoid this sort of formulation in favor of the view that Republicans appeal to the nonminorities, while the Democrats appeal to the minorities. In these terms, the Republicans can be thought of as a coalition of the nonpoor. Whites, nonunion families, Protestants, Northerners, and those outside the central cities.

The Republican groups are larger in size than the corresponding Democratic groups, and they generally have a higher turnout, but size and turnout are only two of the components of a group's contribution to a party's coalition. The Democrats' relative advantage is in the third component, namely partisan support, because the pro-Democratic minorities generally give a higher percentage of their votes to the Democrats than the corresponding nonminorities give to the Republicans.

To measure the magnitude of the three components and the total contribution of each group to its party's electoral coalition, a combination of survey data and election returns can be used. The election returns show the total national loyalty each party received, and they can be also used in conjunction with census data to estimate the national turnout. A survey based upon a carefully drawn national sample can be used to measure the components for each of the separate groups. Fortunately, the Survey Research Center of the University of Michigan has conducted such a national survey for every presidential election since 1952, so comparable data for the last five presidential elections are now available. Income, race, union membership, religion and place of residence, being relatively objective criteria, present few of the problems associated with measuring attitudes. There are still problems in determining how a person actually voted, even if the person is reinterviewed after the election as these respondents were. Some of them evidently reported that they had voted when in fact they did not, and a small percentage reported that they voted for the winner when they actually voted for the loser. The raw survey data have been adjusted with the use of national election data to compensate for these two types of error.[16] The numbers will still contain some random sam-

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[15] The same formula could be used to study contributions of resources other than votes, such as financial contributions and time of volunteers. In that case, the turnout would measure the resource provided by a given group to either party, and the loyalty would measure the proportion of the resource that went to a given party.

[16] The *national turnout* and *loyalty* figures are from the *Statistical Abstract of the United States,* 1970, pp. 354 and 368. The *group size* figures were taken directly from the Survey Research Center's sample. The *group loyalty* and *turnout* figures were adjusted so that taken together with the group sizes they conform with the national loyalty and turnout statistics. The adjustment procedure is an iterative process by which a contingency table with given marginal distributions can be transformed into a table with specified marginal distributions while preserving the nature and strength of the association, as measured by appropriate criteria. The procedure is that developed by Frederick Mosteller, "Association and Estimation in Contingency Tables," *Journal of the American Statistical Association,* 63 (March, 1968), pp. 1–28 (see especially pp. 6–8). For a similar application of this adjustment procedure see David Butler and Donald Stokes, *Political Change in Britain* (New York: St. Martin's Press, 1969), pp. 283ff. The sizes of the national survey samples were 1,899 in 1952; 1,762 in 1956; 1,181 before weighing and 1,954 after weighing in 1960; 1,571 in 1964; and 1,557 in 1968. Pairwise deletions were used with missing data to preserve sample size, which means that a person was deleted from a count only when some information relevant to that particular count was missing.

Table 1. The Democratic Coalition, 1952–1968

	Percentage Contribution						= (Size						× Turnout						× Loyalty)						÷ (NT×NL)	
Year	P	B	U	C	S	CC	P	B	U	C	S	CC	P	B	U	C	S	CC	P	B	U	C	S	CC	NT	NL
1952	28	7	38	41	20	21	36	10	27	26	28	16	46	23	66	76	35	68	47	83	59	57	55	51	63	44.4
1956	19	5	36	38	23	19	25	9	26	25	29	14	40	23	64	72	39	63	47	68	55	53	52	55	60	42.0
1960	16	7	31	47	27	19	23	10	25	25	34	13	46	31	60	74	50	74	48	72	66	82	52	65	64	49.7
1964	15	12	32	36	21	15	19	11	23	26	28	12	45	42	69	72	49	65	69	99	80	75	58	74	63	61.1
1968	12	19	28	40	24	14	16	11	24	26	31	10	44	51	61	68	53	63	44	92	51	61	39	58	62	42.7
Column	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26

Year	Percentage Deviation in Loyalty to Democrats					
1952	+2	+38	+14	+12	+10	+ 6
1956	+5	+26	+13	+11	+10	+13
1960	−2	+22	+16	+32	+ 2	+15
1964	+8	+38	+19	+14	− 3	+13
1968	+1	+49	+ 8	+18	− 4	+15
Column	27	28	29	30	31	32

P	poor (income under $3,000/yr.)
B	Black (and other nonwhite)
U	union member (or union member in family)
C	Catholic (and other non-Protestant)
S	South (including border states)
CC	central cities (of 12 largest metropolitan areas)
NT	National turnout
NL	National loyalty to Democrats

pling error, but most of them are probably within three or four per cent of their true value.

The Democrats. Table 1 gives the data for the Democratic coaliiton in terms of the selected groups. Along the top of the table is written the formula for a group's contribution to a party, and within each block of the table are listed the different groups by year. Directly under the block for loyalty is a block which shows how a group's loyalty differs from the national loyalty.

1. *The Poor.* The poor are defined to be those with an annual family income less than $3000.[17] Column 1 of Table 1 shows that the contribution of the poor to the Democratic coalition has fallen dramatically: from 28 per cent of Democratic votes in 1952 to only 12 per cent in 1968. The main reason their contribution has fallen is that their size has shrunk over these years, from 36 per cent of the total adult population to 16 per cent (column 7). This decrease in the "poor" is partly due to the fact that $3000 per year is a more restrictive definition for the 1960's than it is for the 1950's because of inflation. Much of the decrease, however, is due to gains in real income.[18] Since their turnout has been steady but low at about 45 per cent, (Column 13) and their loyalty (Column 19) has been about 45 per cent Democratic (except in 1964), the decrease in the contribution of the poor to the Democrats is due almost solely to the dwindling size of this group.

The curious fact about the voting pattern of the poor is that, contrary to popular belief, they are not distinctly loyal to the Democrats. Column 19 shows that only in 1964 did they give the Democrats a majority of their votes, and Column 27 shows that to within a few percentage points, their vote generally divides the same as does the entire nation's vote. In brief, the poor are *not* part of the Democratic coalition. The working class may be loyal, but the poor are not.

2. *The Blacks.* The Blacks are another story altogether. Their contribution has grown substantially from 5–7 per cent in 1952–1960 to 12 per cent in 1964 and 19 per cent in 1968 (Column 2). This increase in their contribution to the total Democratic vote is not due to an increase in the number of the Blacks, since their size has been almost constant at about 10 per cent of the population throughout this period (Column 8). It is their turnout that has increased dramatically, nearly doubling between 1952 and 1968. The biggest jumps came in 1960 and 1964 as Blacks began to exercise their franchise in greater numbers, not only in the South but throughout the nation. The Blacks have always been very loyal to the Democrats (Column 20), especially in relation to voting patterns of other groups (Column 28). Curiously enough, this overwhelming loyalty of the Blacks was present in 1952, but not in Stevenson's second effort or in Kennedy's campaign. It returned to and exceeded its previous high level in the campaigns of Johnson and Humphrey. Only in 1968, however, did high loyalty and moderately good turnout combine to produce a Black contribution to the Democrats that was strikingly larger than their population percentage. Of course, the low national loyalty to the Democrats in 1968 also helped account for the high percentage of all Democratic votes that were contributed by Blacks in that year.

3. *The Unions.* Union members and their families provide yet a different story. They contributed more than a third of all Democratic votes in the 1950's, but in the 1960's their contribution fell slightly to a still very respectable figure of 28 per cent (Column 3). This large contribution arises because about a quarter of all adults are in union families (Column 9), they have a turnout close to the national average (Column 15 versus Column 25), and they consistently vote more Democratic than the nation as a whole (Column 29). The gradual decline of the union contribution to the Democratic party is due to a slight decline in their relative size (Column 9) and a large drop in their loyalty in 1968 (Column 21).

A comparison of the Black contribution and the union contribution is revealing, provided one bears in mind that people in both groups vote the way they do for many reasons other than their race and union membership. In 1960 the union voters contributed more than four times as many votes to the Democrats as the Blacks did, but by 1968 the unions contributed only one and a half times as many votes as the Blacks (Column 3 versus Column 2). So until recently the voters from union families made a much, much larger contribution than the Blacks, and they still make a significantly greater contribution.

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[17] This definition is not very satisfactory because inflation and rising median income have changed the social meaning of any fixed income level. It does have the virture, however, of being easy to define and comprehend.

[18] The magnitude of inflation in these years is indicated by the Consumer Price Index. Using 1957–59 = 100, the figures are 92.5 for 1952, 94.7 for 1956, 103.1 for 1960, 108.1 for 1964, and 121.2 for 1968. The figures were prepared by the Bureau of Labor Statistics and are reported by the Council of Economic Advisors in *Supplement to Economic Indicators Historical and Descriptive Background* (1964) and *Economic Indicators* (January, 1970).

4. *The Catholics.* The Catholics have formed a large and reliable segment of the Democratic coalition. They have always provided more than a third of the Democratic votes (Column 4), even though they are only a quarter of the population (Column 10). Part of the reason is their exceptionally high turnout (Column 16). The other part of the reason is their steadfast loyalty (Column 22) which has always been more than 10 per cent greater than that of the population as a whole (Column 30). Included in the Democratic coalition with the Catholics are the Jews, whose numbers are small, but whose turnout and loyalty have consistently been very high.

Of course a special year for the Catholics was 1960, when John Kennedy was the Democratic nominee. In that year the Catholics gave more than four-fifths of their vote to the Democrats (Column 22), thereby providing nearly half of all the votes the Democrats got in 1960 (Column 4).

5. *The Southerners.* The South, including the border states, offers something of a surprise. Voters from this region have provided roughly one-fourth of all Democratic votes (Column 5). The size of the group has held steady (Column 11), while the turnout has increased since 1956 (Column 17), partly because of the increased turnout of the Blacks in the South. The surprise is that overall, Southerners have not been exceptionally loyal to the Democrats since 1956 (Column 23). In the Eisenhower elections they split their vote down the middle, and since then have voted within a few percentage points of the national Democratic average (Column 31).

It appears from these figures that the South is moving away from the Democrats. The Southerners were about 10 per cent more pro-Democratic than the nation as a whole in the 1952 and 1956 elections, but only 2 per cent more in 1960; in 1964 and 1968 they voted 3 per cent and 4 per cent *less* Democratic than the country as a whole did (Column 31). This trend has occurred despite the countertrend of Black Southerners, who are very loyal Democrats, to increase their turnout in those same years. The Democratic loss is not necessarily the Republican gain, for in 1968 a third of the White Southern votes went to Wallace (not shown in the table), accounting for slightly more than half of all Wallace's votes.

6. *The Central Cities.* Only about 10 per cent of the nation's population now lives in the central cities of the dozen largest metropolitan areas, a figure that has been falling slowly since 1952 (Column 12). Central city dwellers used to have a slightly better than average turnout (Column 18), and since 1956 they have been 13 per cent to 15 per cent more Democratic than the nation as a whole (Column 32). These trends make the central cities an important but slowly decreasing part of the Democratic coalition, having provided about 20 per cent of the Democratic votes in the 1950's and about 15 per cent of the Democratic votes in the last two elections (Column 6).

A person may, of course, belong to more than one of these overlapping groups. For this reason the contributions and sizes of the groups listed on Table 1 can add up to more than 100 per cent. Some of the Democratic groups overlap more or less than would be expected from their sizes alone. Thus, for instance, the Blacks constitute a higher proportion of the population of the South than of the North, while on the other hand, the Blacks represent a lower proportion of Catholics than they do of Protestants. Typically, the overlap of two given Democratic groups has a higher loyalty than the average of the loyalties of the two given groups.[19] For example, in 1968, Catholic union members had a loyalty of 65 per cent which is actually greater than the loyalty of either the Catholics (61 per cent) or the union members (51 per cent).

Some people belong to none of the six overlapping groups listed on Table 1. Actually about one-fifth to one-fourth of the adult population is neither poor, Black, unionized, Catholic, Southern, nor in central cities. This residual group tends to vote Republican, as would be expected, and in fact votes about twenty per cent more Republican than the nation as a whole.

One could go on almost indefinitely examining the partisan support provided by people with different attributes. But one additional group merits particular attention: that is the young. Those between 21 and 29 years of age are about 18 per cent of the voting age population, yet they provide only about 14 per cent of all Democratic votes. There are two reasons for this curious fact. First, the young have a turnout record about 15 per cent below the national average. Second, the young are simply not loyal to the Democrats. In fact, in four of the last five presidential elections the young have given the Democrats the same proportion of their vote (within 3 per cent) as has the whole country. In 1968 the young with their weak party loyalties, were slightly pro-Wallace compared to the nation as a whole. So like the poor, the young have not been part of the loyal Democratic coalition.[20] Now that the 18 to 21 year olds have been enfranchised, the size of the under-30 group will increase. Their turnout is likely to remain below average, but it is uncertain whether their pattern of not being loyal to either major party will continue.

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[19] This empirical generalization is substantiated by the 1968 data for the five actual segments of the Democratic coalition. It does not follow automatically from the definitions, however. In fact, the overlap hypothesis that the intersection of two loyal groups is more loyal than the average of the two groups is closely related to the cross-pressure hypothesis that a person who is a member of one Democratic group and one Republican group is likely to be less loyal to both parties than either group membership alone would predict.

The Republicans. The Republican coalition can be thought of as consisting of the overlapping majorities that are the precise complements of the minorities that describe the Democratic coalition. This makes the Republican coalition a combination of the nonpoor, Whites, nonunion families, Protestants, Northerners, and those outside the central cities. Virtually everyone is in at least one of these overlapping groups, and most people are in four or five of them.

Most of the facts about how these groups contribute to the Republican party follow directly from what has already been said about how their complements contribute to the Democratic party. For example, the fact that the poor are no more loyal to the Democrats than is the nation as a whole implies that the nonpoor cannot be especially loyal to the Republicans. Because of these relationships we can discuss only a few of the more salient facts and let Table 2 tell the rest of the story of how the Republican coalition is put together.

1. *The Nonpoor.* Those with incomes greater than $3000 have contributed an increasing proportion of all Republican votes since 1952 (Table 2, Column 1), mainly because more and more people have crossed this line over the years (Column 7). But as was just pointed out, the nonpoor do not deviate much from the national average in their loyalty to the Republicans (Column 27).

2. *The Whites.* Virtually all Republican votes come from Whites (Column 2). Even in 1960 when a quarter of the Black vote went to Nixon, 97 per cent of Nixon's votes came from Whites, while in 1968, 99 per cent of Nixon's votes came from Whites. Whites are about 90 per cent of the population (Column 8) and they vote 1 per cent to 3 per cent more Republican than the nation as a whole (Column 28). This is just the obverse of the statement that the Blacks, who are about 10 per cent of the population, vote heavily Democratic.

3. *The Nonunion Families.* Nonunion families have voted 5 per cent or 6 per cent more Republican than the nation as a whole from 1952 to 1964 (Column 29). This figure takes on real importance when combined with the fact that three-quarters of all adults are in families without a union member (Column 9).

4. *The Protestants.* The Protestants are also about three-quarters of the population (Column 10), and have usually voted about 5 per cent more Republican than the nation as a whole (Column 30). The special year of course, was 1960 when a Catholic candidate opposed a Protestant for President and 90 per cent of the Republican votes came from Protestants (Column 4).

5. *The Northerners.* The Northerners have an unusually high turnout (Column 17), relative to the turnout in the South. When it comes to loyalty, however, the Northerners have never been more than 3 per cent pro-Republican in these five presidential elections, and twice have not been pro-Republican at all (Column 31).

6. *Outside the Central Cities.* Those living outside the central cities of the twelve largest metropolitan areas have slowly increased from about 85 per cent of the total population in the 1950's to 90 per cent in 1968 (Column 12). Their turnout, however, is only about average, and their loyalty is only slightly more Republican than the nation as a whole (Column 32), partly because they are so close to equaling the whole nation.

The Party Coalitions

The most obvious fact about American party coalitions is that they are very loose. They are loose first in the sense that most group loyalties are not total. Except for Blacks, none of the twelve groups that have been examined in the Tables ever gave more than 80 per cent of their votes to one party. Second, the coalitions are loose in the sense that group loyalties are not constant from one election to the next. Finally, when a group's loyalty shifts it is as likely to shift in response to a national trend as it is for reasons specific to the group. Indeed, again with the exception of Blacks, each of the groups usually divided their votes no more than 15 per cent differently than did the nation as a whole.

The data from Table 1 and Table 2 do not point to an election in these years which suddenly shifted the nature of the coalitions and

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[20] In 1968 the young contributed 13 per cent to the Democratic coalition by having a size of 18 per cent, a turnout of 47 per cent and a loyalty of 40 per cent.

Table 2. The Republican Coalition, 1952–1968

	Percentage Contribution						= (Size						× Turnout						× Loyalty)						÷ (NT×NL)	
Year	NP	W	NU	P	N	NCC	NP	W	NU	P	N	NCC	NP	W	NU	P	N	NCC	NP	W	NU	P	N	NCC	NT	NL
1952	75	99	79	75	87	84	64	90	73	74	72	84	72	67	61	58	73	61	56	57	61	61	57	57	63	55.1
1956	84	98	78	75	84	89	75	91	74	75	71	86	67	64	58	56	69	60	59	59	63	62	60	60	60	57.4
1960	83	97	84	90	75	90	77	90	75	75	66	87	70	68	65	61	71	63	50	51	55	63	50	52	64	49.5
1964	89	100	87	80	76	91	81	89	77	74	72	88	67	66	61	60	68	63	40	42	45	44	38	40	63	38.5
1968	90	99	81	80	80	92	84	89	76	74	69	90	65	63	62	60	66	62	44	47	46	49	47	45	62	43.4
Column	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26

Year	Percentage Deviation in Loyalty to Republicans					
1952	+1	+2	+6	+6	+6	+2
1956	+1	+1	+5	+4	+2	+2
1960	0	+1	+5	+13	0	+2
1964	+1	+3	+6	+5	−1	+1
1968	0	+3	+2	+5	+3	+1
Column	27	28	29	30	31	32

NP	nonpoor (income over $3,000/yr.)
W	White
NU	nonunion
P	Protestant
N	Northern (excluding border states)
NCC	not in central cities of 12 largest metropolitan areas
NT	National turnout
NL	National loyalty to Republicans

ıen set the pattern for the elections to follow.
n Key's terms, none of these elections is an ob-
ious choice for a critical election, although
ıere have been secular realignments. These
radual shifts have been in different factors for
ifferent groups: loyalty of the South, size of
ıe poor and central cities dwellers, and both
ırnout and loyalty of the Blacks. There have
so been dramatic short-term shifts, for in-
ance by the Catholics.

The data on party identification provide an-
ther point of view about what has been hap-
ening to the party system. In each year the re-
ondents were asked, "Generally speaking, do
u usually think of yourself as a Republican, a
emocrat, an Independent, or what?" The re-
ılts in Table 3 show that the party identifica-
n has remained almost stable since 1952.
either major party has gained strength, and
e Democrats still have a considerable edge
er the Republicans. A new factor in 1968
as an increase in the number of Independents
29 per cent of the national sample. This in-
ease is not due solely to the appeal of George
allace, because fully 28 per cent of those who
d not vote for Wallace thought of themselves
Independents in 1968.

ble 3. Percentage of Party Identification, 1952–68

Party	Democrat	Independent	Republican
1952	47	22	27
1956	44	24	29
1960	46	23	27
1964	51	23	24
1968	44	29	25

(Note: Several per cent "apolitical" and "don't
ow" in each year are not shown).

The third-party coalition put together by
allace can be measured just as the coalitions
the other two parties have been. In 1968
allace received 14 per cent of the national
te. The only one of the twelve groups that
ve him a great deal more than this was the
uth (including the border states) which had
loyalty to Wallace of 28 per cent. The Catho-
s gave Wallace only 9 per cent of their vote,
e central city dwellers 8 per cent, and the
n-Whites a mere 1 per cent. The other cate-
ries did not have much of a net impact. The
or gave him 16 per cent of their vote, com-
red to 13 per cent from the nonpoor; the
ion families gave him 15 per cent, compared
13 per cent from the nonunion families; and
young gave him 18 per cent, compared to
13 per cent from those 30 and older.

All in all, there have been and still are significant differences in the electoral coaliitons of the two major parties, despite Wallace's claim that there isn't a dime worth of difference in what they stand for. Race, union membership, religion and place of residence still matter. The Democrats usually get major contributions of votes from the Blacks, the union members, the Catholics and those in central cities. The Republicans usually have slightly greater appeal for the Whites, the nonunion families, the Protestants, and those outside the central cities. The poor and the young, however, divided their votes just about the same way as did the nation as a whole and were not really part of any party's electoral coalition.

Some Strategic Considerations

There is little a party can do to increase the size of a demographic group, but there is much it can do to try to increase its turnout and loyalty. A major consideration for a party in deciding whether to appeal to a given group is what might be called the group's "elasticity of response." The question is not whether a group will be receptive to an appeal, but whether a greater or lesser appeal will make much difference in the behavior of the members of the group. From a strategic point of view, the problem is to use scarce resources in the best way, presumably to maximize the chances of victory. If a group has a nearly constant response, independent of the intensiveness of the appeal to it, there is little sense in devoting many resources to that group. But if a group's response is sensitive to the magnitude of the efforts to appeal to it, then a party would do well to devote scarce resources to increase this appeal.

A good example of how a group can respond to a particular appeal is provided by the Catholics. In 1960, the Democrats nominated a Catholic for President, and the proportion of the Catholic vote that went to the Democrats went up almost 30 per cent from 53 per cent in the previous election to 82 per cent. In the same four year period the nation as a whole increased its loyalty to the Democrats by only 8 per cent. Thus the Catholics' response to the appeal of a Catholic candidate was highly elastic. Unfortunately for the Democrats, it was also elastic in the other direction as well. In the next two presidential elections, when Protestants were again nominated, the deviation in loyalty of the Catholics to the Democrats fell back almost to its previous levels (Table 1, column 3).

Another approach is for a party to direct some of its resources to increase the turnout of a group, through such means as a registration drive. If loyalty is to be increased, voters must be converted from another party, but if turnout is to be increased, they need only be mobilized. The mobilization strategy has the disadvantage that a newly mobilized voter who votes for the party is only half as valuable as a voter who is newly converted from the other party of a two-party system. The mobilization strategy has the further disadvantage that the newly mobilized voter may not vote for the intended party. Mobilization is sometimes relatively inexpensive, however, and it can often be accomplished without antagonizing other groups in the coalitions. The ideal opportunity for a mobilization strategy by a party arises when a group has high loyalty combined with low turnout. Such has been the case with the Blacks, and the Democrats have acted accordingly. If there is new evidence that the young will depart from their previous pattern of behavior and start to be loyal to a given party, that party would be wise to use a mobilization strategy aimed at the young.

The expression of a group's contribution in terms of its size, turnout, and loyalty can be used to analyze hypothetical strategic choices involving any division of the population. The first step is to notice that the contribution of any group plus the contribution of its complementary group is always one. Using the definition of contribution, this gives

$$\frac{s_g t_g l_g}{t_n l_n} + \frac{s_c t_c l_c}{t_n l_n} = 1.00$$

where s, t and l are size, turnout and loyalty respectively and the subscripts g, c and n signify the group, its complement, and the nation. The national turnout can be expressed as a function of the size and turnout of the group and its complement,

$$t_n = s_g t_g + s_c t_c,$$

and the size of the complement can be expressed in terms of the size of the group itself,

$$s_c = 1 - s_g.$$

Substituting the last two equations into the first one, and solving for the national loyalty gives what might be called the strategic equation,

$$l_n = \frac{s_g t_g l_g + (1 - s_g) t_c l_c}{s_g t_g + (1 - s_g) t_c}.$$

The strategic equation has six variables. A typical application would assume that the values of several of them are known from previous measurements or extrapolations, and would ask what would be the effect of varying one or two of the remaining variables. For example, if on knew the size, turnout, and loyalty of a grou he could determine what levels of turnout an loyalty would have to be achieved from th people *outside* the group in order to get a give percentage of the national vote.

As a concrete example, consider as a singl group the people that Scammon and Wattenberg[21] emphasize, the "unyoung, unpoor an unblack." Using 1968 SRC data and the previous definitions, the size of this group is 62 pe cent and its turnout is 70 per cent, while th turnout of all the others is 50 per cent. Substituting these values in the strategic equation an solving for l_c gives

$$l_c = .328 l_n - .228 l_g.$$

Thus if a party could count on 55 per cent c the vote of the "unyoung, unpoor an unblack," it would still need at least 39 per cer of the other voters to win a majority in a tw party race. In a three party race in which could count on 50 per cent of the "unyoun unpoor and unblack," it would still need 34 pe cent of the other voters to get 45 per cent c the national vote.

Other problems can also be analyzed wit the strategic equation. If an appeal to a grou increases its loyalty by a given amount, ho much can the loyalty of the other voters d crease before the break-even point is reache If the turnout of a given group is increased b a given amount, how much does this increase party's percentage of the national vote? Final a generalized form of the strategic equation ca be used to analyze similar questions involvi the partitioning of the population into mo than two nonoverlapping groups.[22]

This paper has described how the contrib tions that different groups make to a party's t tal strength can be specified. It has also show how these contributions can be broken dow into their three components—size, turnout, a loyalty. Through the examples of select groups, the actual magnitude of these contrib tions and their components for each of the la five presidential elections have been present and discussed with the hope that measuri these previous coalitions can be helpful in t task of understanding what may come next. leave that greater task to others, whether th be scholars, politicians, or voters.

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[21] Scammon and Wattenberg, *Real Majority*.

[22] If the nation is partitioned into mutually haustive and nonoverlapping groups (such as inco levels), the strategic equation is

$$l_n = \frac{\Sigma s_i t_i l_i}{\Sigma s_i t_i}.$$

Turnout and Representation in Presidential Primary Elections*

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After Carl Albert adjourned the 1968 Democratic National Convention *sine die*, a good many Democrats and Democrat-watchers left Chicago's tear-gassed streets and stink-bombed hotels convinced that something was terribly wrong with the traditional way the party had gone about choosing its presidential nominee. The Democrats' subsequent loss of the election gave them about the only benefit the losing party ever gets: a chance to make major changes in its structure and procedures without having to clear them with an incumbent President.

Surprisingly, the party did not let the chance slip away. Chicago's "winners" joined the "losers" to launch the most ambitious intraparty effort for presidential nominating reform since the Republicans adopted the bonus-vote apportionment principle more than a half-century ago.[1] Executing a mandate from the convention, the National Chairman, Senator Fred Harris of Oklahoma, established two commissions in early 1969: the Commission on Party Rules, chaired by Representative James O'Hara of Michigan; and the Commission on Party Structure and Delegate Selection, chaired until early 1971 by Senator George McGovern of South Dakota and since then by Representative Donald Fraser of Minnesota. Both commissions urged a number of drastic changes in the party's accustomed ways, and the Democratic National Committee has incorporated most of them in the rules governing the 1972 national convention.

The content, rationale, implementation, and impact of these new rules are topics outside the central concerns of this paper. For present purposes we need note only that the McGovern-Fraser Commission's most fundamental charge against the established delegate-selecting procedures was that they make the convention *unrepresentative*. A review of the commission's hearings and deliberations confirms my participant-observer's impression that, while its majorities did not really face up to the enduring complexities and dilemmas in the theory and practice of representation,[2] they found the traditional conventions unrepresentative in two main respects. First, they are *demographically* unrepresentative. The commission declared that certain significant elements of the citizenry—blacks, women, and young people are the ones specifically mentioned in the report—should be represented among the delegates in proportions that "bear a reasonable relationship to the group's presence in the population";[3] and past conventions have fallen woefully short of this ideal. Second, the conventions are *preferentially* unrepresentative—that is, they do not accurately represent the issue and candidate preferences of the party's rank-and-file. Many 1968 dissidents argued that the victories of Eugene McCarthy and Robert Kennedy in the presi-

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[1] It is worth noting that the bonus-vote reform was adopted by the Republicans after the angry divisions and charges of rigging in their 1912 convention and their subsequent schism and loss of the presidency. The Democrats, being in power at the time, did not follow suit for several decades. See Paul T. David, Ralph M. Goldman, and Richard C. Bain, *The Politics of National Party Conventions* (Washington, D.C.: The Brookings Institution, 1960), pp. 165–168.

[2] The literature on representation is as vast as its intellectual puzzles and institutional mazes are difficult. I have found especially helpful the work of Hanna Fenichel Pitkin, especially her *The Concept of Representation* (Berkeley and Los Angeles: University of California Press, 1967). Also useful are many of the papers in J. Roland Pennock and John W. Chapman, eds., *Representation: Nomos X* (New York: Atherton Press, 1968).

[3] Commission on Party Structure and Delegate Selection, *Mandate for Reform* (Washington, D.C.: Democratic National Committee, 1970), Guideline A–2, pp. 40, 26–28; see also the summary in *Congressional Quarterly*, November 28, 1969, pp. 2418–2419.

dential primaries showed that the great majority of ordinary Democrats wanted an immediate end to the Vietnam war and to the Johnson-Humphrey leadership that was perpetuating it. Yet the convention refused to adopt a meaningful antiwar plank, and gave its nomination to prowar Humphrey rather than to antiwar McCarthy or McGovern. The commission's new guidelines governing the state parties' selection of convention delegates are intended, some say, to prevent any future convention from being so out of tune with the social composition, issue stands, and candidate preferences of the party's grass roots.

A few party leaders, of course, feel that *no* delegate convention, however reformed, can ever be as representative, demographically or preferentially, as a national presidential primary. Thus in June, 1968 Senator Mike Mansfield introduced the most recent of a long series of proposals for establishing a nationwide primary election to precede or even replace the conventions.[4] Like its predecessors, the Mansfield bill did not get out of committee, but the idea is far from dead. And whatever may be the prospects for a national primary, it is clear that state presidential primaries are likely to play an important role in the 1970s: at the present writing no fewer than seven states (Arkansas, Maryland, New Mexico, New York, North Carolina, Rhode Island, and Tennessee) have newly added presidential primaries for 1972. This makes a total of twenty-two states and the District of Columbia with presidential primaries, by far the largest number since 1924.[5]

Most political scientists who have written on the question feel that the present "hybrid" or "mix" of primaries and conventions is desirable and that neither element should be totally abandoned in favor of the others.[6] But for present purposes the significant fact is the tacit agreement among most of the convention reformers, national-primary advocates, and defenders of the convention-primary mix that the presidential primaries—especially those organized on the Oregon model[7]—are presently the most representative element in the mix, the principal standard by which the representativeness of the other procedures for selecting and mandating convention delegates should be judged.

This paper is intended to help test the empirical validity of this widely held assumption. First, however, let us set the inquiry in the context of what we already know about the differences in turnouts in primary and general elections for lesser offices and the impact of those differences on the representative quality of the electorates.

I. "Surge and Decline" in Primary and General Elections

In a seminal article published more than a decade ago, Angus Campbell noted that "one of the most dependable regularities of American politics is the vote decline in off-year congressional elections. The turnout in the off-year elections is invariably smaller than in the presidential elections which they follow, usually by a margin of over 25 per cent of the presidential vote."[8] Moreover, Campbell declared, the off-year electorates are *not* small but representative versions of the presidential-year electorates. They differ significantly in several respects. Using data from the Survey Research Center's studies of voters and nonvoters in the 1956 and 1958 elections, he compared the "core voters" (those who voted in both elections) with the "peripheral voters" (those who voted in 1956 but not in 1958). He found that the core voters were demographically quite unrepresentative of the peripheral voters: they scored significantly higher on the socioeconomic dimensions of age, education, income, and status of occupation; and even more impressive differences appeared on various dimensions of political interest and involvement.

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[4] The first proposal was made by Woodrow Wilson in his initial address to Congress in 1913. Others have since been made by Senators George Smathers (1952), Paul Douglas (1953), Estes Kefauver (1953), William Langer (1955), William Proxmire (1960), and Margaret Chase Smith (1961).

[5] For a useful listing of the state presidential primaries and their outcomes from 1912 to 1964, see James W. Davis, *Presidential Primaries: Road to the White House* (New York: Thomas Y. Crowell Company, 1967), pp. 278–305.

[6] See, for example, Nelson W. Polsby and Aaron B. Wildavsky, *Presidential Elections: Strategies of American Electoral Politics*, 2nd ed. (New York: Charles Scribner's Sons, 1968), pp. 238–239; Davis, *Presidential Primaries: Road to the White House*, pp. 269–271; Gerald Pomper, *Nominating the President* (New York: W. W. Norton & Company, 1966), pp. 230, 275–276; and Frank J. Sorauf, *Party Politics in America* (Boston: Little, Brown and Company, 1968), p. 273.

[7] The most praised features of the Oregon presidential primary are the requirements that: (1) *all* serious presidential contenders are placed on their particular party's ballot unless they specifically state that they are not candidates, which is thought to make it difficult for any bona fide contender to duck a hard contest; and (2) the delegates are legally bound to support the preferential poll's winner for one or more convention ballots. At present the primaries in Maryland, New Mexico, North Carolina, Tennessee, and Wisconsin have similar, though not identical, provisions.

[8] Angus Campbell, "Surge and Decline: A Study of Electoral Change," *Public Opinion Quarterly*, 24 (Fall, 1960), 397–418. The article has been reprinted in Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *Elections and the Political Order* (New York: John Wiley and Sons, Inc., 1966), pp. 40–62. The latter version is cited here, and the quotation in the text is from page 51.

Campbell's analysis deals explicitly only with turnouts in general elections for President and U.S. Representatives, but it offers a promising framework for understanding the consequences of the even greater differences in turnout between primary and general elections delineated by several scholars. V. O. Key, for example, found that in fifteen nonsouthern states from 1926 to 1952 the average voting turnout rate in gubernatorial primaries was 31 percent, while the turnout in the ensuing general elections averaged well over 50 per cent.[9] Arthur C. Wolfe collected turnout data for all states' primary and general elections for governor, U.S. Representative, and U.S. Senator for the period 1952–1960, and found primary-general differences comparable to Key's.[10] And my own comparison of turnout rates for primary and general elections for governor and U.S. Senator for the period 1962–1968 shows the same direction and magnitude of differences in turnout.[11]

It seems reasonable to expect, however, that *presidential* primaries will be different. After all, they play a significant, though not decisive, role in choosing the candidates for our highest and most visible office.[12] Certainly any hotly-contested primary held when a party's nomination is not foreclosed—e.g., in Wisconsin and West Virginia in 1960, in California in 1964, and in New Hampshire, Wisconsin, Oregon, and California in 1968—receives much more attention from the state and national communications media than all but a handful of gubernatorial and senatorial primaries.

To test this expectation, I have assembled turnout data on the 72 presidential primaries held from 1948 through 1968 in which voters had the opportunity to vote directly (in presidential preference polls) or indirectly (in choosing among slates of delegates pledged to particular aspirants, as in California) for their preferences for the parties' national nominees. For all 72 cases the mean turnout in the primaries was only 27 per cent compared with a mean turnout in the states' ensuing general presidential elections of 62 per cent. This "surge" of 35 percentage points is notably larger than any we have considered up to this point.

The figures, however, are misleading, for in many of the 72 cases there was no contest, or no serious contest, in one or both parties' primaries, and in others the contestants did not include a major national contender in either or both parties. Accordingly, it seems appropriate to look closely only at the genuinely competitive presidential primaries. For selecting such primaries I have used the following criteria: (1) two or more candidates or candidate-pledged slates appear on each party's ballot; (2) at least one major national contender appears on each party's ballot; and (3) in neither party does the winner get more than 80 per cent of the votes. Eleven primaries meet all these criteria, and data on their turnouts are presented in Table 1.

All eleven competitive presidential primaries summarized in Table 1 were concentrated in two years, 1952 and 1968.[13] The table makes clear that even the competitive primaries produced a primary-to-general election "surge" of 30 percentage points, which is somewhat larger than the comparable surges for gubernatorial and senatorial election pairs described above.

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[9] V. O. Key, Jr., *American State Politics: An Introduction* (New York: Alfred A. Knopf, 1956), pp. 134–138.

[10] Wolfe found a mean turnout rate in all primary elections for governor of 32 per cent, as compared with a mean turnout rate in general gubernatorial elections of 53 per cent. The corresponding means for U.S. Senator were 30 per cent in the primaries and 54 per cent in the general elections; and for U.S. Representatives, 27 per cent in the primaries and 52 per cent in the general elections. See Arthur C. Wolfe, *The Direct Primary in American Politics* (unpublished Ph.D. thesis in political science, University of Michigan, 1966), Table 4.6, p. 102. For comparable data on turnout in primary elections and general elections for U.S. Representatives from 1938 to 1956, see Julian F. S. Foster, *Congressional Primaries* (unpublished Ph.D. thesis in political science, University of California, Los Angeles, 1963), Ch. IV.

[11] My data show that in all election-pairs in which both parties' primaries were contested and both parties presented candidates in the general election, the mean turnouts were: gubernatorial primaries, 34 per cent, gubernatorial general elections, 57 per cent; senatorial primaries, 32 per cent, senatorial general elections, 57 per cent. A somewhat revised version of these data is presented in "Parties in State Politics," in Herbert Jacob and Kenneth N. Vines, eds., *Politics in the American States: A Comparative Analysis*, 2nd ed. (Boston: Little, Brown and Company, 1971), Table 3, p. 98.

[12] Cf. the discussions of the presidential primaries' role in determining who gets the nominations in David, Goldman, and Bain, *The Politics of National Party Conventions*, Ch. 10; Polsby and Wildavsky, *Presidential Elections*, pp. 83–86; Davis, *Presidential Primaries: Road to the White House*, Chs. 4, 10; and Richard M. Scammon and Ben J. Wattenberg, *The Real Majority* (New York: Coward-McCann, Inc., 1970), Chs. 7–10.

[13] The criterion of not more than 80 per cent of the vote for the winner excluded most primaries: in 1948 either Dewey or Truman or both won over 80 per cent of the vote in one or both contests in every state, and the same was true for Eisenhower in 1956, Nixon in 1960, and Johnson in 1964.

Table 1. Turnout in Competitive Presidential Primaries and Ensuing General Elections, 1948–1968

State	Year	Per Cent Turnout in Primary*	Per Cent Turnout in Gen. Elect.*	Difference
California	1952	42	68	26
Nebraska	1952	39	73	34
New Hampshire	1952	37	78	41
Ohio	1952	25	70	45
Oregon	1952	45	67	22
Pennsylvania	1952	19	66	47
South Dakota	1952	41	73	32
Nebraska	1968	43	63	20
New Hampshire	1968	38	71	33
Oregon	1968	55	66	11
Wisconsin	1968	49	69	20
Mean turnout		39	69	30

* Turnout percentage is calculated, as in most studies, by dividing the total number of votes cast by the estimated population of voting age.

Sources: Primary election returns, 1948–1964, are taken from James W. Davis, *Presidential Primaries: Road to the White House* (New York: Thomas Y. Crowell Company, 1967), pp. 296–305. All general election returns and primary election returns for 1968 are taken from various volumes of Richard M. Scammon, ed., *America Votes* (Washington, D.C.: Governmental Affairs Institute and Congressional Quarterly). Estimates of populations of voting age for the states are taken from the *Statistical Abstract of the United States, 1969* (Washington, D.C.: Bureau of the Census, 1969), Table 534, p. 369.

In short, in turnout, at least, presidential primaries are species of the genus primary elections, not of the genus presidential elections.

But what are the consequences, if any, of their persistently low turnouts for the demographic and preferential *representativeness* of presidential primaries?[14] Studies of electorates in other types of low-turnout elections give us some clues. We have already noted Angus Campbell's conclusion that the "core voters" in off-year congressional elections are socioeconomically of higher status and politically more interested and involved than the "peripheral voters" who join them in presidential-year elections. Campbell did not ask whether the two electorates' preferences on issues and candidates differ significantly, though he did suggest that standing party loyalties are relatively more powerful in the low-turnout elections.[15] Our studies of voters and nonvoters in the Wisconsin gubernatorial primary and general elections of 1964 and 1966 concluded that the primary electorates were demographically unrepresentative but preferentially representative of both the general-election-only voters and the nonvoters in all four elections.[16]

So the question becomes: is the representative quality of presidential primaries simply an instance of that of primary elections in general; or are significant differences wrought in presidential primaries by the greater importance and visibility of the presidency and by some special characteristics of presidential primary laws? Let us see.

II. Voters and Nonvoters in Two 1968 Presidential Primaries

The Data. The data for the following analysis of voters and nonvoters in presidential primaries are taken from sample surveys of participants and nonparticipants in the primaries held in New Hampshire and Wisconsin in 1968. I would, of course, like to have had survey data for the Indiana, Nebraska, Oregon, California, and South Dakota primaries as well, but they were not available. Fortunately, however, not only were the New Hampshire and Wisconsin studies available, but they also had several other advantages: both were conducted by highly reputable survey research organizations;[17] comparisons of their results with the election returns suggest that both samples were well drawn; and the primaries' outcomes—both predicted and recorded—were, correctly or not, widely regarded as a major influence on Lyndon Johnson's decision not to run for reelection.[18]

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[14] As the terms are used in this paper, "demographic representativeness" denotes the degree of likeness between the distributions of socioeconomic characteristics among the representatives (in this case, voters in the primaries and other low-turnout elections) and the corresponding distributions among the represented (in this case, the eligible but nonvoting citizens). "Preferential representativeness" denotes the degree of likeness between the distributions of preferences for candidates and attitudes on public issues among the representatives and the corresponding distributions among the represented.

[15] Campbell, "Surge and Decline . . . ," pp. 53–57. See also Angus Campbell and Homer C. Cooper, *Group Differences in Attitudes and Votes* (Ann Arbor: Survey Research Center, University of Michigan, 1956), Ch. IV.

[16] Austin Ranney and Leon D. Epstein, "The Two Electorates: Voters and Nonvoters in a Wisconsin Primary," *The Journal of Politics*, 28 (August, 1966), 598–616; and Austin Ranney, "The Representativeness of Primary Electorates," *Midwest Journal of Political Science*, 12 (May, 1968), 224–238.

[17] The New Hampshire study was conducted by Audits and Surveys, Inc., of New York City. A statewide probability sample of registered Democratic and Republican voters was interviewed, yielding a total of 1143 usable interviews. The data are made available by the Inter-University Consortium for Political Research, and the Social Science Data and Program Library Service of the University of Wisconsin. The Wisconsin study was conducted by the Wisconsin Survey Research Laboratory, using a statewide clustered area probability sample of persons of voting age and yielding a total of 573 usable interviews. The data are made available by the WSRL.

When considering the findings reported below, one should bear in mind two differences in the New Hampshire and Wisconsin studies. First, the New Hampshire data came from interviews taken *before* the March 12 election (in the period February 12–19), while the Wisconsin data came from interviews taken just *after* the April 2 election. Second, the New Hampshire sample consisted entirely of registered voters, while the Wisconsin sample, like those in the 1964 and 1966 gubernatorial primary studies, was drawn from the state's entire population of voting age, registered or not.

Mode of Analysis. The data were analyzed as follows. Since most (though not all) political leaders and commentators feel that presidential primaries should represent each *party's* rank-and-file, and not all citizens regardless of party identification or "membership," the two samples were first divided into Republicans and Democrats (those expressing some degree of preference for one party or the other) and Independents (those expressing no party preference of any degree).[19] The New Hampshire sample yielded 656 Republicans, 380 Democrats, and 62 Independents; in Wisconsin the numbers were 197 Republicans, 281 Democrats, and 52 Independents. Next, each party-identification group in New Hampshire was divided, according to expressed intention to vote or not vote in the upcoming primary, into "intenders" and "nonintenders."[20] Comparable divisions were made in the Wisconsin sample, except that the party identifiers were divided into "voters" (those who reported having voted in the primary) and "nonvoters" (those who reported not having voted in the primary). This resulted in the following twelve basic analytic groups:

New Hampshire

Republican intenders (RI): 485
Republican nonintenders (RN): 171
Democratic intenders (DI): 251
Democratic nonintenders (DN): 129
Independent intenders (II): 33
Independent nonintenders (IN): 29

Wisconsin

Republican voters (RV): 134
Republican nonvoters (RN): 63
Democratic voters (DV): 158
Democratic nonvoters (DN): 123
Independent voters (IV): 28
Independent nonvoters (IN): 24

As in the 1966 study, in each state each party's participants (intenders or voters) were compared with its nonparticipants (nonintenders and nonvoters) on a number of socioeconomic, attitudinal, and behavioral measures.[21] For each intraparty participant/nonparticipant comparison the null hypothesis was that the distribution in the sample does *not* support the

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[18] Cf. Theodore H. White, *The Making of the President 1968* (New York: Atheneum Publishers, 1969), Ch. 4; and Lewis Chester, Godfrey Hodgson, and Bruce Page, *An American Melodrama: The Presidential Campaign in 1968* (New York: Dell Publishing Company, 1969), pp. 1–10.

[19] Both studies used the Survey Research Center's familiar battery of questions for eliciting party identification.

[20] All New Hampshire respondents were asked before the primary, "As of now, how certain are you that you will vote in the primary this year?" The replies were distributed as follows:

Response	*Number*	*Per Cent*
Very certain	796	69.6
Somewhat certain	155	13.6
Not at all certain	107	9.4
Will not vote	54	4.7
Don't know	23	2.0
Not ascertained	8	0.7
Total	1143	100%

Since the election returns showed that 67.0 per cent of New Hampshire's registered voters voted in the primary, it seemed reasonable to classify those answering "very certain" as intenders, and those giving the other answers as nonintenders. After removing the respondents whose party identifications were not ascertained, there remained 769 intenders and 329 nonintenders.

[21] Some colleagues have suggested that the representativeness of presidential primary electorates would be better tested by comparing the party identifiers who vote in a state's primary with *all* who identify with that party in the state: for example, RI/RI + RN rather than RI/RN. They correctly point out that this approach would narrow the participant/nonparticipant differences reported in this study, and thus enhance the primaries' claim to be truly representative. However, I have chosen the straight participant/nonparticipant comparison because I believe it more accurately operationalizes the concept of representation held by most party leaders who regard the primaries as the nominating system's most representative element. From what they say and do, on the McGovern-Fraser Commission and elsewhere, it seems clear that most leaders assume a sharp distinction between the "representatives" (the convention delegates, the primary voters) and the "represented" (the party's grass-roots supporters). If, as I believe, this concept of representation permeates most current political dispute about the proper organization and role of primaries and conventions, then accepting it rather than substituting a concept which regards the representatives as part of the represented seems likely to make our empirical findings more relevant to those disputes.

conclusion that participants in the population differ significantly from nonparticipants, and "significance," in the usual statistical sense, is assessed by the chi-square test, with the .05 level stipulated.

III. Demographic Representativeness

The two data sets analyzed here were produced by studies of different populations conducted by different research organizations at different times under different research designs. Consequently, on some dimensions we have identical or closely comparable measures, but on others we do not. It is therefore necessary to divide the findings under each heading into those produced for both states by comparable measures and those produced for each state by noncomparable measures. The findings with respect to demographic representatives are summarized schematically in Table 2.

The data summarized in Table 2 show that the party-identifying primary participants in both states differed significantly from their nonparticipating fellow partisans on a number of socioeconomic dimensions, while there were substantially fewer significant differences between participating and nonparticipating independents. We shall observe this same pattern on most of our other measures of representativeness as well.

There were too few Blacks in either state's sample to produce any statistically significant differences in the black-white distributions of the six participant-nonparticipant pairs, and in Wisconsin there were no significant differences in the national origins of voters and nonvoters. However, in the light of the McGovern-Fraser Commission's finding that both young people (defined by the commission as those under thirty) and women were badly underrepresented in the 1968 Democratic National Convention,[22] the figures in Table 3 on age and sex distributions are of some interest. A scan of Table 3 leaves no doubt that, while young people were only slightly underrepresented in the New Hampshire primaries, they were significantly underrepresented in both parties' primaries in Wisconsin. Women, on the other hand, were only slightly underrepresented in the New Hampshire primaries, while in Wisconsin they outnumbered men in both primaries. This may reflect the facts that in both states higher proportions of men than of women were registered voters, but in New Hampshire the sample con-

Table 2. Socioeconomic Characteristics of Presidential Primary Participants and Nonparticipants

Socioeconomic Characteristic	New Hampshire			Wisconsin		
	RI/RN	DI/DN	II/IN[a]	RV/RN	DV/DN	IV/IN[a]
Comparable Measures						
Age	+[b]	0[c]	0	+	+	0
Sex	0	0	0	0	0	0
Education	+	0	0	0	0	0
Occupation	0	0	0	+	0	0
Income	+	0	0	+	+	0
Religious preference	0	0	0	0	0	0
Frequency of church attendance	+	+	0	0	+	+
Size of place	0	0	0	0	0	0
Union membership	0	0	0	0	0	0
Non-comparable Measures: N.H.						
Self-assigned social class	0	0	0			
Non-comparable Measures: Wis.						
Membership in party clubs				+	0	0
Membership in church clubs				+	+	+
Membership in fraternal organizations				+	0	0
Membership in social clubs				+	0	0
Membership in PTAs				+	+	0
Length of residence in community				+	+	0
Home ownership				+	+	0
Original nationality on father's side				0	0	0

[a] For definitions of the groups compared, see the text, p. 110

[b] Participants score significantly higher than nonparticipants.

[c] Difference between participants and nonparticipants not significant at the .05 level.

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[22] *Mandate for Reform*, pp. 27–29.

Table 3. Distributions of Presidential Primary Participants and Nonparticipants, by Age and Sex

	New Hampshire							Wisconsin					
	RI	RN	DI	DN	II	IN		RV	RN	DV	DN	IV	IN
Age							Age						
21–24	2%	5%	2%	1%	3%	0%	21–25	3%	16%	4%	17%	4%	17%
25–29	5	5	2	3	6	7	26–30	9	8	9	10	4	8
30–34	6	6	5	8	15	0	31–35	5	5	9	13	7	8
35–44	18	12	22	16	27	17	36–45	31	13	24	15	25	21
45–54	19	14	21	16	9	31	46–55	21	17	22	15	18	8
55–64	17	22	17	19	15	21	56–65	14	16	14	9	21	25
65 and over	29	31	26	34	18	21	66 and over	17	25	18	19	21	13
n. a.	4	5	5	3	7	3	n. a.	0	0	0	2	0	0
	100%	100%	100%	100%	100%	100%		100%	100%	100%	100%	100%	100%
N	485	171	251	129	33	29		134	63	158	123	28	24
	$X^2=10.65$* df=6 p=n.s.		$X^2=7.18$ df=6 p=n.s.		$X^2=10.11$ df=6 p=n.s.			$X^2=17.41$ df=6 $p<.01$		$X^2=19.17$ df=6 $p<.01$		$X^2=4.47$ df=6 p=n.s.	
Sex							Sex						
Female	37%	43%	37%	42%	36%	24%	Female	52%	59%	57%	55%	39%	46%
Male	63	57	63	58	64	76	Male	48	41	43	45	61	54
	100%	100%	100%	100%	100%	100%		100%	100%	100%	100%	100%	100%
	$X^2=1.67$ df=1 p=n.s.		$X^2=.77$ df=1 p=n.s.		$X^2=.59$ df=1 p=n.s.			$X^2=.49$ df=1 p=n.s.		$X^2=.03$ df=1 p=n.s.		$X^2=.04$ df=1 p=n.s.	

* "Not ascertained" row eliminated before calculating chi squares.

sisted of registered voters, while the Wisconsin sample consisted of resident adults. Or it may reflect the greater proportion of women to men in Wisconsin (47 to 53 per cent) than in New Hampshire (43 to 57 per cent) in 1968. Or it may reflect a greater liberation from male chauvinist attitudes about political participation in Wisconsin than in New Hampshire. The data do not tell us, but they do suggest that the 1968 presidential primaries did a better job of representing women than of representing people under thirty, and a better job of representing both groups than that accomplished by the national conventions.

Otherwise, the data confirm that presidential primary electorates, like those in gubernatorial primaries, are demographically quite unrepresentative of the nonparticipating party identifiers: they are older, of higher status in income and occupation, and more active in a variety of civic, religious, and political organizations.

IV. Representativeness in Partisanship

Lester W. Milbrath declares that "at least nine studies in three or more countries have shown that *persons who strongly identify with or intensely prefer a given party are more likely to participate actively in the political process.*"[23] The explanation usually given is that most citizens find the political world hard to fathom and are unwilling or unable to pay the high costs of acquiring full information. Hence the party labels attached to particular candidates together with the citizens' standing party identifications provide the most powerful cues for making choices. In high-stimulus presidential elections, intensity of party identification is more strongly related to the way a person would vote if he voted than to the likelihood of his voting; but in low-stimulus off-year congressional elections, strength of partisanship is also strongly related to voting turnout.[24]

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[23] Lester W. Milbrath, *Political Participation* (Chicago: Rand McNally & Company, 1965), p. 52, emphasis in the original.

[24] Statements of the underlying causes for the partisanship-participation relationship are given by Don R. Bowen, *Political Behavior of the American Public* (Columbus, Ohio: Charles E. Merrill Publishing Company, 1968), p. 129; and in Leon D. Epstein's discussion of the general role of party labels in "structuring the vote," in *Political Parties in Western Democracies* (New York: Frederick A. Praeger, Publishers, 1967), Ch. IV. The Survey Research Center's findings on the relationship in presidential elections are set forth in Angus Campbell, Gerald Gurin, and Warren E. Miller, *The Voter Decides* (New York: Harper and Row, 1954), pp. 107–110, esp. Table 7.11; and in Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: John Wiley & Sons, Inc., 1960), pp. 142–143. The SRC's findings on the corresponding relationship in off-year congressional elections are given in Campbell and Cooper, *Group Differences in Attitudes and Votes,* Ch. IV.

Table 4. Intensity of Party Identification and Primary Participation, by Party and State

Wisconsin

Voted in primary?	Republicans			Independents	Democrats		
	Strong Rep.	Weak Rep.	Ind. Rep.		Strong Dem.	Weak Dem.	Ind. Dem.
Yes	75%	63%	70%	54%	60%	53%	59%
No	25	37	30	46	40	47	41
	100%	100%	100%	100%	100%	100%	100%
N	59	88	50	52	68	139	74
		$X^2 = 2.53$ df = 2 p = n.s.				$X^2 = 1.23$ df = 2 p = n.s.	

New Hampshire

How certain to vote in primary?	Republicans			Independents	Democrats		
	Strong Rep.	Weak Rep.	Ind. Rep.		Strong Dem.	Weak Dem.	Ind. Dem.
Very certain	77%	72%	69%	53%	75%	60%	62%
Other*	23	28	31	47	25	40	38
	100%	100%	100%	100%	100%	100%	100%
N	304	275	77	62	142	156	82
		$X^2 = 2.20$ df = 2 p = n.s.				$X^2 = 8.33$ df = 2 $p < .02$	

* Somewhat certain, not at all certain, will not vote.

One may reasonably derive from these findings about general elections the hypothesis that strong party identifiers are more likely than weak identifiers to vote in low-stimulus primary elections. Yet our gubernatorial-primary studies did not confirm this hypothesis: we found that, while Strong Republicans are somewhat more likely than Weak Republicans to vote in such primaries, among Democrats there was even a weak *inverse* relationship between intensity of party identification and participation in the primary.[25] The data summarized in Table 4 suggest that, with one exception, intensity of partisanship played no stronger a role in impelling people to vote in the 1968 presidential primaries than it had in the 1964 and 1966 Wisconsin gubernatorial primaries. Of the party groupings shown in the table, only the New Hampshire Strong Democrats voted in the primary in a significantly higher proportion than their less intensely identified fellow partisans. Some doubt arises about the significance of even this finding since the New Hampshire respondents were all registered voters, in itself an indication of a general interest and involvement in politics.[26] The doubt is increased by the finding that when a control is imposed for the respondents' expressed interest in the *particular* 1968 primary (as opposed to a general interest in all elections), the New Hampshire Democrats' relationship between intensity of partisanship and primary participation/nonparticipation weakens and fails to meet our test of significance at the .05 level. In sum, strong partisans of both parties are only slightly more likely than weaker partisans to vote in presidential primaries, and the relationship between intensity of arty identification and voting in such primaies is much weaker than that between partisanhip and voting in presidential general lections.

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[25] See the studies mentioned in footnote 16.

[26] For two somewhat different analyses of the interaction of institutional requirements, including registration laws, and levels of psychological motivation as they affect voting turnout, see *The American Voter*, Ch. 11; and Stanley Kelley, Jr., Richard E. Ayres, and William G. Bowen, "Registration and Voting: Putting First Things First," *American Political Science Review*, 61 (June, 1967), 359–379.

V. Preferential Representativeness

The McGovern-Fraser Commission conends, correctly in my view, that demographic epresentativeness is an important standard by hich to measure the viability of the convenions, the primaries, and the whole presidential ominating process. Quite aside from the qualty, acceptability, and electoral appeal of the ominee chosen, if important segments of the arty's identifiers and supporters or of the comunity at large feel that the nominating process nd machinery do not want their presence or ear their voices, the legitimacy of the nominaion and of the process itself is likely to be hallenged and perhaps denied. If that happens, he consequences for the party—and, indeed, or the whole political system—could be catatrophic.

Even so, *preferential* representativeness eems to many a still more important standard. For even if the primaries were dominated by nd the conventions composed largely of middle-class white males, if they consistently wrote platforms and chose candidates desired by the bulk of the party's supporters, the probabilities of major revolts against the nominating process or its products would be greatly reduced. So the question of the preferential representativeness of presidential primary electorates is nearer the heart of the matter than anything we have considered up to now.

Issue Orientation. One prime element of preferential representativeness is issue orientation: the degree of correspondence between the feelings of representatives and the feelings of those represented about what government should and should not do. The 1968 studies give us considerable data on the preferential representativeness of presidential primary electorates, although, unfortunately, none resulting from comparable measures used in both states. The New Hampshire study dealt mainly with questions about salient national political issues, asking a total of ten questions tapping views about the importance of issues, four questions concerning issues of national domestic policy, and five questions about national policy in Vietnam.

There were no statistically significant differences within any of New Hampshire's three participation pairs concerning the importance of the various issues, but some noteworthy differences in policy preferences did appear. The less striking differences came over domestic issues, as is shown by the answers arrayed in Table 5. Table 5 reveals that, while on each domestic issue a majority of the Republican primary participants agreed with a majority of the Republican nonparticipants, the participants had higher proportions taking conservative stands on all four issues, and on two the proportions were sufficiently different to produce chi-square scores significant at the .05 level. Democratic participants and nonparticipants, on the other hand, held quite similar views on three of the four domestic issues; but on the fourth, a majority of the participants opposed national protection of equal job opportunities for Negroes, while a plurality of the nonparticipants supported such protection. On this important issue, then, Democratic primary participants were distinctly *un*representative.

The four questions on the Vietnam war, however, produced the most significant participant/nonparticipant issue differences that have appeared in this study and in its 1964 and 1966 predecessors. The differences are shown in Table 6. One pattern evident in Table 6 is that Republican participants and nonparticipants agreed that present and past Democratic administrations had done things all wrong: Pluralities of both groups felt that we should not have entered the Vietnam fighting in the first place, and majorities of both disapproved of President Johnson's current handling of the situation. Pluralities of both groups also opposed any unconditional halt of the bombing of North Vietnam; but on the general question of what policy the nation should follow, the Republican participants were distinctly more hawkish than the nonparticipants: 45 per cent favored a stronger stand, and 42 per cent favored efforts to end the fighting; among the nonparticipants, on the other hand, only 30 per cent favored a stronger stand and 50 per cent favored efforts to end the fighting.

Over on the Democratic side, the participant/nonparticipant differences on the war issues were even more pronounced. A majority of the Democratic intenders approved our initial entry into the war, while a plurality of the nonintenders disapproved it; the participants approved their party chief's handling of the war by a margin of 56 per cent to 31 per cent, while the nonparticipants supported him by only 43 per cent to 39 per cent. On questions of future policy, the participants, like their Republican counterparts, were somewhat more hawkish: 53 per cent approved taking a stronger stand, compared with 42 per cent of the nonparticipants; and 86 per cent opposed

Table 5. Opinions on Domestic Issues of New Hampshire Presidential Primary Intenders (I) and Nonintenders (N), 1968

Question	Reps.		Dems.		Inds.	
	I	N	I	N	I	N
"Do you think the Government in Washington should help towns and cities provide education for grade and high school children, or do you think this should be handled by the states and local communities?"						
Govt. in Washington should help	14%	26%	38%	36%	27%	24%
Leave to states & localities	79	58	54	48	67	62
Don't know, not ascertained	7	16	8	16	6	14
	100%	100%	100%	100%	100%	100%
Number of cases	485	171	251	129	33	29
	$X^2=15.85$*		$X^2=.02$		$X^2=.05$	
	df=1		df=1		df=1	
	$p<.001$		$p=$n.s.		$p=$n.s.	
"Do you think the Government in Washington should see to it that every person has a job and a good standard of living, or should it let each person get ahead on his own?"						
Govt. in Washington should help	15%	25%	39%	35%	21%	31%
Let each get ahead on his own	78	61	55	57	73	48
Don't know, not ascertained	7	14	6	8	6	21
	100%	100%	100%	100%	100%	100%
	$X^2=10.35$		$X^2=.44$		$X^2=1.03$	
	df=1		df=1		df=1	
	$p<.01$		$p=$n.s.		$p=$n.s.	
"Do you think civil rights leaders are trying to push too fast, are going too slowly, or are they moving at about the right speed?"						
Too fast	64%	59%	61%	53%	67%	69%
Too slowly	4	7	6	10	0	7
About the right speed	26	28	28	29	30	14
Don't know, not ascertained	6	6	5	8	3	10
	100%	100%	100%	100%	100%	100%
	$X^2=2.19$		$X^2=2.21$		$X^2=4.09$	
	df=2		df=2		df=2	
	$p=$n.s.		$p=$n.s.		$p=$n.s.	
"Do you think the Government in Washington should see to it that Negroes get fair treatment in jobs, or should these matters be left to the states and local communities?"						
Govt. in Wash. should guarantee fair job treatment for Negroes	32%	35%	43%	49%	30%	31%
Leave to states & localities	58	52	50	41	64	52
Don't know, not ascertained	10	13	7	10	6	17
	100%	100%	100%	100%	100%	100%
	$X^2=.75$		$X^2=1.75$		$X^2=.01$	
	df=1		df=1		df=1	
	$p=$n.s.		$p=$n.s.		$p=$n.s.	

* "Don't know, not ascertained" rows eliminated before calculating chi squares.

Table 6. Opinions on Vietnam War Issues of New Hampshire Presidential Primary Intenders (I) and Nonintenders (N), 1968

Question	Reps.		Dems.		Inds.	
	I	N	I	N	I	N
ıg in Vietnam, or should we have stayed out?" — Do you think we did the right thing in getting into the fight-						
Did the right thing	45%	37%	59%	44%	58%	38%
Should have stayed out	47	52	38	47	39	45
Don't know, not ascertained	8	11	3	9	3	17
	100%	100%	100%	100%	100%	100%
Number of cases	485	171	251	129	33	29
	$X^2 = 1.69$* df = 1 p = n.s.		$X^2 = 4.81$ df = 1 $p < .05$		$X^2 = .54$ df = 1 p = n.s.	
'Do you approve or disapprove of the way President Johnson s handling the situation in Vietnam?"						
Approve	20%	27%	56%	43%	27%	34%
Disapprove	61	50	31	39	45	52
Don't know, not ascertained	19	23	13	18	28	14
	100%	100%	100%	100%	100%	100%
	$X^2 = 5.74$ df = 1 $p < .02$		$X^2 = 4.49$ df = 1 $p < .05$		$X^2 = .01$ df = 1 p = n.s.	
"Which of the following do you think we should do now in Vietnam?"						
Pull out entirely	9%	14%	8%	9%	12%	10%
Keep our soldiers in Vietnam, but try to end the fighting	33	36	26	30	27	28
Take a stronger stand, even if it means invading North Vietnam	45	30	53	42	48	48
Don't know, not ascertained	13	20	13	19	13	14
	100%	100%	100%	100%	100%	100%
	$X^2 = 9.28$ df = 2 $p < .01$		$X^2 = 2.53$ df = 2 p = n.s.		$X^2 = .04$ df = 2 p = n.s.	
"Which of the following do you think we should do now with regard to the bombing of North Vietnam?"						
Halt the bombing immediately, even if it doesn't lead to negotiations	7%	6%	8%	9%	15%	0%
Halt the bombing immediately and see if it leads to negotiations	12	20	6	14	12	3
Halt the bombing now only if North Vietnam agrees to negotiate	42	35	40	33	45	38
Do not halt the bombing until North Vietnam surrenders	34	30	43	35	24	59
Don't know, not ascertained	5	9	3	9	4	0
	100%	100%	100%	100%	100%	100%
	$X^2 = 8.02$ df = 3 $p < .05$		$X^2 = 8.47$ df = 3 $p < .05$		$X^2 = 10.53$ df = 3 $p < .02$	

* "Don't know, not ascertained" rows eliminated before calculating chi squares.

any unilateral bombing halt, compared with 68 per cent of the nonparticipants.

Thus in both parties, but more among the Democrats than the Republicans, the New Hampshire primary participants were distinctly more hawkish than the nonparticipants, although on only two questions did a plurality of the intenders take a position different from that favored by a plurality of the nonintenders. We shall consider below some possible consequences of these differences for the primaries' outcomes.

The Wisconsin respondents, unfortunately, were asked only about state and local issues. As in New Hampshire, there was no significant difference within any of the three pairs of voters and nonvoters about the relative importance of the various issues facing the state. On four questions dealing with environmental issues, the RVs and RNs and the IVs and INs showed no significant differences, but on one of the four the DVs were significantly more willing than the DNs to "pay somewhat more in taxes if the money could be used to help clean up Wisconsin lakes and streams." On three questions about the state's welfare programs and their beneficiaries, the DVs and DNs and the IVs and INs showed no significant differences; but significantly more RVs than RNs disagreed with the proposition that "people who need assistance from a social welfare agency are really good people who have had a bad break in their lives."

Since the small wars in Madison were then second in public concern only to the large war in Vietnam, the survey also contained four questions about various aspects of the way the University of Wisconsin was handling its student affairs, and these evidently touched some Republican (though no Democratic or Independent) nerves. The two groups of Democrats and the two groups of Independents all agreed that the University had been too lenient in making and enforcing rules governing student activities, but the differences in the proportions holding these views did not meet the .05 test. On all four questions, however, the RVs were significantly more critical of University permissiveness than were the RNs. And on the question, "To what degree do you approve or disapprove of the gradual relaxing of regulations governing student hours, visiting privileges in residence halls, and apartment living at the U.W.?," 54 per cent of the RNs expressing an opinion approved relaxing the regulations, while 80 per cent of the RVs *dis*approved. Had these data been available to Madison students in September, 1970, perhaps many more would have worked to get out the vote—especially i the Republican primaries, and for candidate identified with other causes as well as wit peace. But they were not, and U.W. students like the Adult Establishment, were less inactiv in the 1970 November general election than i the September primaries.

Candidate Preferences. Many commentator and most political leaders hold that the prime if not the only, function of a party's presidential nominating process is to produce the candidate most likely to win in November. Some however, believe that its prime function is t produce the candidate most preferred by th party's ordinary adherents whatever may b their reasons or the wisdom of their choice In either case, perhaps the most critical test o a convention's or a primary's preferential representativeness is whether the participant choose the candidate the party's identifier most want. The record of the 1968 Ne Hampshire and Wisconsin primaries on thi test is set forth in Tables 7 and 8.

Table 7 makes it quite clear that the New Hampshire Republican intenders' preferences not only closely resembled those of the Republican nonintenders, but also came very close to the choices actually registered in the primary balloting. This finding both increases our confidence in the sample and suggests that the Republican primary intenders well represented the candidate preferences of nonintending registered Republicans: more than three-quarters of both groups wanted Nixon, and Nixon they got.

The New Hampshire Democratic intenders, on the other hand, not only gave Lyndon Johnson a significantly larger proportion of their votes than did the Democratic nonintenders (and Eugene McCarthy a correspondingly smaller proportion), but they were also substantially more pro-Johnson than were the voters in the primary. These discrepancies may, of course, result from a biased sample or a poorly-conducted set of interviews. But they may support the opinion of some commentators that a considerable swing away from Johnson to McCarthy took place in the three weeks between the date of the last interviews (February 19) and primary day (March 12).[27] If that

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[27] Chester, Hodgson, and Page state that private polls commissioned from Quayle Associates by the Johnson organization showed a substantial surge for McCarthy in the final three weeks of the campaign: *An American Melodrama*, pp. 110–112. White cites other private polling data showing a steady rise in McCarthy support from January on: *The Making of the President 1968*, p. 89.

Table 7. Candidate Preferences of Intenders and Nonintenders in the 1968 New Hampshire Presidential Primary

Candidate	Election Returns	Sample							
		All		Reps.		Dems.		Inds.	
		I	N	I	N	I	N	I	N
Johnson	18.4%	20.8%	19.9%	0.4%	0.6%	63.1%	47.7%	20.0%	16.0%
McCarthy	18.1	8.4	15.1	0.9	0.6	24.3	37.8	4.0	4.0
Nixon	52.2	54.0	45.4	77.6	76.1	4.5	4.5	60.0	36.0
Rockefeller	7.0	5.9	6.2	8.7	9.7	0.5	2.8	4.0	0
Romney	1.1	7.1	6.9	9.8	9.7	1.3	0	8.0	20.0
Other	3.2	3.8	6.5	2.6	3.3	6.3	7.2	4.0	24.0
	100%	100%	100%	100%	100%	100%	100%	100%	100%
N	159,402	707	291	460	155	222	111	25	25[c]
				$X^2 = .30$[a] df = 3 p = n.s.		$X^2 = 7.88$[b] df = 2 $p < .02$			

[a] Chi square calculated for Republican candidates only.
[b] Chi square calculated for Democratic candidates only.
[c] Too few cases for chi-square calculation.

was the case, we cannot say that the data for Democratic intenders and nonintenders in Table 7 prove that the New Hampshire Democratic primary electorate in 1968 was unrepresentative of the party rank-and-file. Perhaps the best judgment for our null hypothesis here is the old Scottish verdict of "not proved."

The Wisconsin data in Table 8 show a striking correspondence between the preferences reported by the voters in our sample and those cast in the actual election. The sample nonvoters were not directly asked their preferences, however, so we have to approach the representativeness question indirectly. Each respondent was asked to rate each of the candidates on a scale from 0 (don't like him at all) to 100 (like him very much). We were thus able to construct a composite rating of each candidate for each of our analytic groups and compare them between groups. The results are set forth in Table 9.

Comparisons of the rank orders of the candidate ratings summarized in Table 9 reveal no significant differences between Republican voters and non-voters. Inspection of the individual composite-rating scores suggests that the RVs liked Nixon and McCarthy better than the RNs did, and liked Johnson and Kennedy less; but

Table 8. Candidate Preferences of Wisconsin Sample and Voters, 1968 Presidential Primary

Candidate	Election Returns	Sample			
		All	Republicans	Democrats	Independents
Johnson	20.7%	20.8%	2.3%	38.1%	13.6%
McCarthy	33.7	34.9	20.9	46.3	40.9
Kennedy	3.8	7.4	2.3	10.9	13.6
Nixon	31.9	31.2	65.1	2.0	27.3
Reagan	4.2	1.7	3.9	0	0
Other	4.2	3.0	4.7	2.0	0
No preference*	1.5	1.0	0.8	0.7	4.6
	100%	100%	100%	100%	100%
N	1,222,855	298	129	147	22

* A "no preference" option was offered on the election ballot as well as on the interview schedule.

Table 9. Ratings of Presidential Candidates by Wisconsin Primary Voters and Nonvoters, 1968

Candidate	REPUBLICANS					DEMOCRATS					INDEPENDENTS				
	Voters			Nonvoters		Voters			Nonvoters		Voters			Nonvoters	
	Rank Order of Votes	Rating	Rank Order of Rating	Rating	Rank Order of Rating	Rank Order of Votes	Rating	Rank Order of Rating	Rating	Rank Order of Rating	Rank Order of Votes	Rating	Rank Order of Rating	Rating	Rank Order of Rating
McCarthy	2	46.3	2	45.9	2	1	57.6	1	55.6	2	1	58.9	1	59.2	1
Johnson	3*	25.2	4	37.2	4	2	52.8	2	44.9	3	3*	30.0	4	32.1	4
Kennedy	3*	30.4	3	44.9	3	3	52.1	3	61.1	1	3*	45.0	3	42.9	3
Nixon	1	66.3	1	62.3	1	4	29.0	4	27.1	4	2	51.8	2	55.0	2

* Tied rank.

both groups' rank orders in ratings were the same, and also the same as the rank order (except for the Johnson-Kennedy tie) that appeared in the RVs' reported votes. So there is little here to suggest that the RVs were significantly unrepresentative of RNs in their attitudes toward and preferences among the candidates. The same is true of the IVs and INs.

The Democrats, however, do not present so neat a pattern. Not surprisingly, the rank order of the DVs' candidate ratings is the same as for their reported votes; but the DNs' individual rating scores and the rank order of their ratings show much greater approval of Robert Kennedy and disapproval of Lyndon Johnson than shown by the DVs. In this sense, then, the DVs distinctly underrepresented the Kennedy sentiment among all Wisconsin Democrats and (strange as it may seem in the light of the election outcome) overrepresented the Johnson strength. We cannot say with confidence to what extent this distortion reflected the respondents' post-election knowledge that Johnson had withdrawn from the national race (as he did on March 31st, two days before the primary) and Kennedy had entered it (on March 16th, seventeen days before the primary). Yet it certainly raises the question of what damage is done to the representativeness of presidential primaries by laws that make it possible—indeed, likely—that only some serious national contenders will be on the ballots and others' names will have to be written in if they are to receive any votes at all.[28]

VI. Wisconsin's Crossovers

In 1968 Wisconsin was the only presidential-primary state using the open primary system: the law provided that every qualified voter was entitled to vote in the primary of either party (but not in both) without publicly disclosing his choice. The La Follette progressives wrote this provision into Wisconsin's first primary laws in the early 1900s, and it has never been effectively challenged or lost its strong popular support.[29]

Most political analysts and party leaders prefer closed primaries to the Wisconsin system. After all, they argue, the candidates' party labels constitute the main signposts guiding many voters through the mazes of American long ballots; hence choosing which aspirants shall and shall not bear its official imprimatur is the most important decision made by any party that seriously contests elections. That being the case, it seems only logical and fair to restrict the right to participate in making a party's nominations to bona fide party "members," however defined. Closed primaries at least try to achieve this goal, but open primaries, like Wisconsin's, are designed to facilitate "crossing over"—voting in one party's primary by adherents of an opposing party or of no party at all.

Several scholars have speculated about the extent, motivation, and impact of crossovers in open-primary elections. Some have pointed to instances in which the total vote in a particular party's primary exceeded its vote in the general election, and then drawn the not unreasonable inference that the discrepancy resulted from extensive crossovers.[30] One motivation often ascribed for crossing over is the desire of the adherents of one party—especially the second party in a modified one-party system—to help the best (or least bad) of the other party's aspirants win its nomination.[31] Another, quite different motivation sometimes ascribed is said to underlie the practice of "raiding"—adherents of one party participating in the other party's primary "to assure the nomination of a weak candidate who can be defeated in the general election."[32]

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[28] In 1968 the New Hampshire Republican primary ballot carried the names of Nixon, Romney (who withdrew from the national contest on February 28, thirteen days before the primary, but whose name remained on the ballot), Harold Stassen, Willis Stone, Herbert F. Hoover, David Watumull, William Evans, Elmer Coy, and Don Dumont. There were also Republican write-in votes for Rockefeller, McCarthy, and Johnson. The Democratic primary ballot carried the names of McCarthy, John Crommelin, Richard Lee, and Jacob Gordon; and write-in votes were recorded for Johnson and Nixon. Under Wisconsin's new Oregon-style law, the state placed on the Republican ballot Nixon, Reagan, Stassen, Romney, and Rockefeller (who withdrew but received write-in votes). McCarthy and Johnson were placed on the Democratic ballot, and there were also write-in votes for Kennedy, Humphrey, and George Wallace. See Polsby and Wildavsky for an illuminating discussion of the strategy of write-in campaigns: *Presidential Elections*, pp. 84–85.

[29] In our study of the 1966 Wisconsin gubernatorial primary we asked our respondents if they favored changing the open primary. 82 per cent favored keeping it as it is, and only 9 per cent favored converting it into a closed primary.

Survey data clearly have many advantages over aggregate data for studying both the extent of and motivations for crossing over. The Wisconsin data in Table 8, for example, show that more than one-fifth of the sample's Republican voters (20.9 per cent) reported crossing over to vote for McCarthy. Adding the 2.3 per cent who voted for Johnson and the 2.3 per cent who voted for Kennedy produces the finding that a full quarter of the Republican voters in the sample voted in the Democratic primary. (Only 2.0 per cent of the Democrats reported voting in the Republican primary, all of them for Nixon.) These voters, moreover, were an important component of McCarthy's overall margin over Johnson and Nixon, as is shown by the data in Table 10. Table 10 makes it clear that, while McCarthy would have defeated Johnson even if only Democrats had voted in the Democratic primary, his Republican-crossover support provided much of his impressive margin: among Democrats alone, McCarthy led Johnson with 48.6 per cent of the sample to 40.0 per cent; but with the Republicans added, he led with 54.9 per cent to Johnson's 34.1 per cent.

Table 10. Party Composition of the Three Leading Candidates' Support in the 1968 Wisconsin Presidential Primary

Party Identification of Voters	Voted for Nixon	Voted for Johnson	Voted for McCarthy
Republican	90%	5%	26%
Democratic	3	90	65
Independent	7	5	9
	100%	100%	100%
Number of cases	93	62	104

Table 11. Reasons Given for Crossing Party Lines, Wisconsin Presidential Primary, 1968

"Why did you cast your primary vote for a candidate for President [not of the party with which you identify]? Does this mean you are moving closer to [the other party], or what?"	Number Responding
No, but felt he was best qualified; his views best coincided with mine; didn't like the other candidates	20
I just consider myself closer to [the other party]	6
No, crossed over to show disapproval of the Vietnam war	2
Yes, *not further specified*	3
Not ascertained	7
Total	38

The total of 38 crossovers in our Wisconsin sample[33] does not constitute an N large enough for any very elaborate analysis. Each respondent who reported crossing over, however, was asked why he or she had done so, and their answers are tabulated in Table 11.

So far as it goes, the distribution of answers in Table 11 strongly suggests that here, at least, we are dealing with a case in which most of the crossovers temporarily deserted their party to register their special approval of a candidate, or a policy associated with a candidate, available only in the other party. McCarthy's Republican supporters clearly did not vote for him because they thought he would be the easiest Democrat for Nixon to beat in November; they did so because they liked him and his opposition to the Vietnam war. This case does not, of course, prove that raiding the opposition's primary to nominate a weak candidate never takes place; but it does nothing to contradict the view that only a handful of ordinary American voters have the high political concern and technical sophistication required to use their primary votes in such a Machiavellian manner.[34]

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[30] Cf. Sorauf, *Party Politics in America*, pp. 205–206. For a more detailed account of crossing over in Wisconsin primaries, see Leon D. Epstein, *Politics in Wisconsin* (Madison: University of Wisconsin Press, 1958), pp. 94–95; and Howard R. Penniman, *The American Political Process* (Princeton, N.J.; D. Van Nostrand Company, Inc., 1962), pp. 85–86.

[31] Sorauf, *Party Politics in America*, p. 206.

[32] V. O. Key, Jr., *Politics, Parties & Pressure Groups*, 5th ed. (New York: Thomas Y. Crowell Company, 1964), p. 391. See also Kay Lawson, *Political Parties and Democracy in the United States* (New York: Charles Scribner's Sons, 1968), p. 134.

[33] They included twenty-seven Republicans for McCarthy, three for Johnson, three for Kennedy, and one for Wallace; and three Democrats for Nixon and one for Rockefeller.

VII. Conclusion

What does the foregoing analysis tell us about the widespread assumption, noted at the outset, that the presidential primaries are the most representative element of our current nominating system and furnish the standard by which the representativeness of its other elements should be judged? At least three of the findings seem most relevant.

First, it is clear that presidential primary electorates, like those in primaries for lesser offices, are demographically quite *un*representative of their nonvoting fellow partisans. They are unrepresentative not only in the same respects in which voters differ from nonvoters in general elections, but also in much the same way that national convention delegates—at least prior to 1972—have been unrepresentative both of their parties' rank-and-file identifiers and of the general population. Consequently, if our prime object is to maximize demographic representation in the presidential nominating system, strict enforcement of rules for selecting convention delegates like those developed by the McGovern-Fraser Commission is clearly a much better bet than expanding the number of state primaries or adopting a nationwide primary.

Second, the existing presidential primaries' claim to a high degree of preferential representativeness is better, though hardly without flaws. Of eight national issue questions asked in New Hampshire, six evoked substantial differences between Republican participants and nonparticipants. In four of the six instances, the differences consisted of the greater intensity of the participants' preferences; in the other two instances, majorities of the participants favored policies substantively at odds with those favored by majorities of the nonparticipants. New Hampshire Democrats had major differences on only three of the issue questions, and all three were differences of intensity, not substance. Eleven questions on state issues were asked in Wisconsin; five produced significant differences between Republican voters and nonvoters, one of which was a disagreement on substance and the others differences in intensity. Hence presidential primary electorates are also unrepresentative in issue orientation, at least to the degree that differences in the intensity with which opinions are held are politically as significant as differences in the direction of those opinions.[35]

Finally, given the fact that in each state some national contenders were on the ballot and others were not, there is no reason to suppose that if all of each party's identifiers had voted in the primaries the outcomes would have been significantly different. But what the outcomes would have been in our samples as well as in the actual elections if *all* the contenders had been on the ballot and if no write-ins were permitted or necessary, we cannot say. Certainly the sharp increase from 1968 to 1972 in the number of states holding presidential preference primaries has not made the rules governing these matters more uniform or rendered interstate comparisons or primary outcomes more meaningful.

This paper, of course, shows only that presidential primaries fall well short of any ideal of perfect representativeness. It touches only indirectly on the larger question of whether or not the primaries are *more* representative than the other institutions of the nominating system. We can speak to that question best by setting alongside the data presented here comparisons of the demographic composition, issue stands, and candidate preferences of local, state, and national convention delegates with those of the nonparticipating party identifiers at each level. For such comparisons we presently have usable data for national convention delegates and party identifiers since 1952, but little or none for state and local convention delegates and their constituents.

The parties' nominating processes in 1972 provide unique opportunities for collecting such data. The McGovern-Fraser Commission's efforts to make all levels of the Democrats' process more representative should yield, at a minimum, far more systematic evidence about the composition and preferences of national, state, and local conventions than ever before available. And the Republicans' decision to make only minimal changes in their procedures should at least provide something approximating control groups for assessing the impact of the Democrats' reform efforts upon the representativeness of their conventions.

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[34] It is worth recalling, in this regard, that our study of the 1966 Wisconsin gubernatorial primary turned up only ten crossovers. Of these, four were Republicans who left their own uncontested primary to vote in the hotly-contested Democratic primary, but six were Democrats who made the opposite move! See Ranney, "The Representativeness of Primary Electorates," pp. 227–228. Both the 1966 and 1968 Wisconsin findings support Sorauf's view that "as for organized 'raiding,' there is precious little evidence to suggest it is more than a worrisome myth": *Party Politics in America*, p. 206.

[35] Leading discussions of the problem posed for theories of democracy and representation by differing intensities with which opinions are held by different members of the community include: Robert A. Dahl, *A Preface to Democratic Theory* (Chicago: University of Chicago Press, 1956), Ch. 4; Henry B. Mayo, *An Introduction to Democratic Theory* (New York: Oxford University Press, 1960), pp. 178–179, 206–212; and Willmoore Kendall and George W. Carey, "The 'Intensity' Problem and Democratic Theory," *American Political Science Review*, 62 (March, 1968), 5–24.

If political scientists take advantage of these opportunities, we should be much better placed than now not only to answer the scientific question raised at the beginning of this paper but also to furnish much of the information decision makers will need to determine whether a national primary, or more state primaries, or reformed party conventions, or some mixture of the three is the best way to give Americans the truly representative presidential nominating process we say we want.

Decision Making in Soviet Cities

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The study of Soviet decision making has traditionally been a difficult task. Soviet authorities do not publicly ask the question, "Who makes the decisions here?" No published Soviet studies of decision making exist; nothing written attempts to describe and analyze a "process" of decision making; no Soviet political science texts provide empirical data covering the entire range of governmental activity on a particular territorial-administrative level. Soviet authorities publish few details about how the Party makes policy and then ensures policy implementation through its control over the governmental-administrative structure known as the system of Soviets. Nor do they provide any real information on how decisions are made by the various governmental-administrative organs and levels which comprise the system of Soviets.

As a result, detailed Western studies of the Soviet decision-making process are scarce. Good luck, in the form of captured Soviet archives, enabled Merle Fainsod to produce *Smolensk Under Soviet Rule*—a classic study of decision making in the province of Smolensk during the 1930s.[1] More recently, Philip Stewart and Jerry Hough produced important studies of Soviet provincial and local politics by relying upon painstaking scholarship and fortuitous research experiences in the USSR.[2] My own study of Soviet municipal politics is based upon a similar combination of good fortune and sustained research over a number of years.[3] I chose to examine the urban level of Soviet politics because Soviet authorities, apparently concerned over the effects of rapid Soviet urbanization, have begun to publish a considerable amount of new information on Soviet urban life and politics, and have made it easier for Western noncommunist scholars to obtain useful interviews with Soviet municipal officials.[4] These research opportunities have made it possible to examine selected aspects of decision making on the urban level, to draw some conclusions about the future development of municipal politics, and to make some broader generalizations about the national Soviet political system.

My choice of four aspects of decision making—budget formulation, the planning process, housing construction and allocation, the staffing of top urban posts—is based upon the greater availability of Soviet data for these types of decisions, as compared to such issue areas as local industry or education. In addition, budget, planning, housing and staffing are probably the four most important municipal decisions which are made in the USSR. Without a plan nothing can be produced or distributed by anyone in any Soviet organization. Budget and plan are closely related—one cannot be conceived nor exist without the other. New and better housing is now a high priority item, and the provision of housing is becoming a major municipal function. A carefully structured system of staffing (*nomenklatura*) facilitates Party control over the entire municipal decision-making process, by ensuring that top municipal posts are staffed by the Party, with men and women who are Party members.

From my examination of the decision-making process in each of these four areas, I conclude that urban autonomy has increased slightly in the 1960s, although Soviet municipalities still remain very much restricted by superior Party and governmental authorities operating under the centralizing principles of Party dominance and dual subordination. The Party remains the directing core of all Soviet organizations, and it determines the regime's priorities. The nature and quality of this Party dominance may be changing, as the educational qualifications and professional expertise of urban Party officials rises, but these changes have not altered the basic fact of Party dominance over decision making. Dual subordination—whereby higher level bodies establish policy guidelines for their subordinate organs at the lower levels and can suspend decisions made by these subordinate organs—remains fully in operation. Dual subordination encourages national uniformity in decision making and it discourages the growth of local parochialism (*mestnichestvo*). Unfortunately, the result has been a highly centralized system which practically has stifled local initiative. Now, as municipal administrators and other local specialists acquire more expertise and influence in making administrative and technical decisions within the municipality, one might expect local initiative to develop in the future.

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[1] Merle Fainsod, *Smolensk Under Soviet Rule* (Cambridge: Harvard University Press, 1958).

[2] Philip D. Stewart, *Political Power in the Soviet Union: A Study of Decision Making in Stalingrad* (Indianapolis: Bobbs-Merrill, 1968): Jerry F. Hough, *The Soviet Prefects* (Cambridge: Harvard University Press, 1969).

[3] B. Michael Frolic, "Soviet Urban Politics," unpublished doctoral dissertation, Cornell University, 1970. See also B. Michael Frolic, "The Soviet Study of Soviet Cities," *Journal of Politics,* 32 (August, 1970), 675–695, and "Municipal Administrations, Departments, Commissions and Organizations," *Soviet Studies,* 22 (January, 1971), 376–393.

[4] For example, in 1968, B. N. Gabrichidze, a Soviet political scientist, produced the first full-length study of Soviet municipal government. A year later, a major work on urban sociological research was published. See B. N. Gabrichidze, *Gorodskiye Sovety deputatov trudyashchikhsya* [City Soviets] (Moscow, Yuridicheskaya literatura, 1968); and Institut konkretnykh sotsialnykh issledovanii AN SSSR, *Sotsiologicheskiye issledovaniya goroda* [Urban Sociological Research] (Moscow, 1969). During two trips to the USSR, in 1965 and 1969, I interviewed 70 officials in ten Soviet cities (Baku, Bratsk, Donetsk, Irkutsk, Leningrad, Minsk, Moscow, Tallinn, Tashkent, and Thorez).

Budget Formulation

The municipal finance department (gorfo) is responsible for the budgeting process within Soviet cities. This process is complex and takes almost a full year to complete. Gorfo is formally accountable to two state organs: the executive committee of the city Soviet (the cabinet of city government), and the Republic Ministry of Finance.[5] In a city of 500,000 inhabitants, gorfo normally will employ about 125 persons, most of them having specialized training in finance or in economics. Gorfo's top staff are usually Party members, although few of its leading cadres have held full-time Party posts. They generally devote their entire careers to the state finance apparatus.[6]

At the start of each year, municipal functional and branch administrations, departments, organizations and commissions[7] submit their budget proposals to gorfo. These proposals must fall within frames of reference already established in prior, informal discussions with gorfo and with the municipal executive committee, using republic and national Party and state control figures as basic guidelines. For example, a municipal trade and public catering administration which may want a large increase in operating revenues and in capital expenditures to build and staff more restaurants, must first consider the Party's announced national commitment to the service sector. (Will there be a significant increase? If yes, are restaurants a high priority item?) Then the priorities of the republic have to be met. (Perhaps other cities have more urgent needs for new restaurants.) Finally, there are other municipal needs to consider. Given the choice of either a new department store or some new restaurants, the executive committee may opt for the department store. After taking such factors into account, the trade and public catering administration will submit its formal budget proposal to gorfo.

Gorfo organizes the various budget proposals into one consolidated municipal draft budget, and this draft budget is submitted in May to the executive committee for discussion. At this stage, gorfo has already consulted with its vertical superior, the republic Ministry of Finance, in order to ascertain just what the republic is prepared to allocate to the municipality (more than 90 per cent of municipal revenues come from republic sources), and to determine how much room there might be for some bargaining between the municipality and the republic later on. Gorfo and the municipal planning commission (gorplan) confer together, to ensure that the draft budget and draft plan are coordinated. The executive committee normally works on the draft budget for two or three months. The chairman of the executive committee (the mayor) takes charge of the budget in the executive committee. The committee meets with the heads of gorfo, gorplan, other city department heads, the chairmen of city district executive committees, and Party officials, to discuss the draft and to make changes.[8] The executive committee then approves the draft and sends it upwards to the republic Ministry of Finance, either directly or through gorfo. "At this point in municipal budget formulation," observed a Tallinn gorfo official, "significant changes in the original draft budget may have been made by the executive committee, with the implicit approval of gorfo and the Ministry."

The draft budget arrives in the Ministry of Finance by August. The Ministry examines the draft and consults with the republic Council of Ministers and with other republic committees and ministries. Budget cuts are invariably made. The Ministry meets with top gorfo offi-

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[5] I am using as examples the cities of Tallinn (Estonia) and Donetsk (the Ukraine), two cities whose immediate governmental superior is the republic level. Smaller, less important cities normally come under the jurisdiction of provinces or districts, the lower links in the Soviet state hierarchy.

[6] See Frolic, "Municipal Administrations, Departments, Commissions and Organizations," for a fuller description of gorfo's structure and its other municipal duties.

[7] Referred to, hereafter, as "municipal administrations."

[8] City districts are governmental-administrative subdivisions in large Soviet cities. In structure they are smaller replicas of their city parent. Moscow has 29 city districts, each with a population of 200,000–250,000 inhabitants.

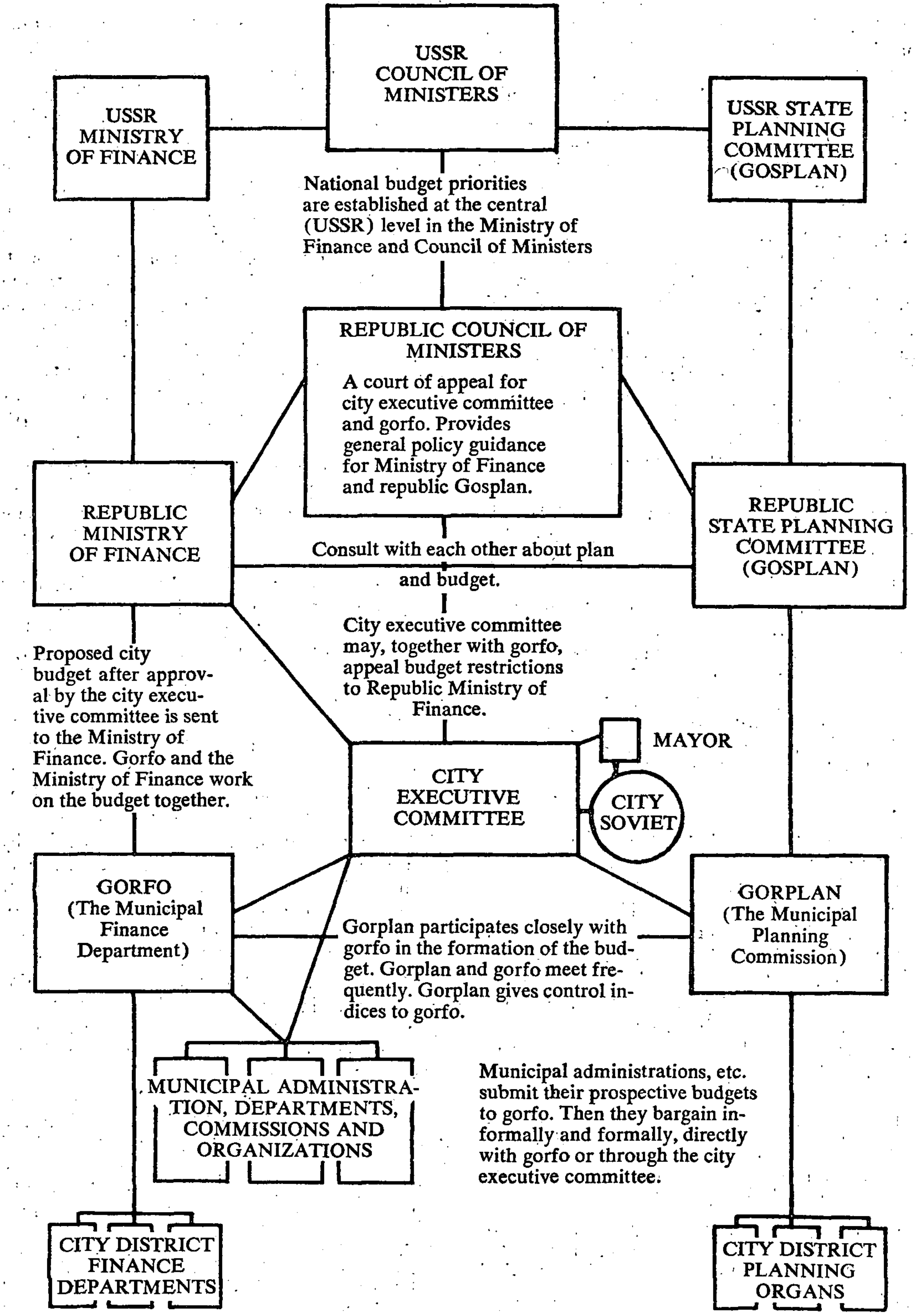
USSR COUNCIL OF MINISTERS
USSR MINISTRY OF FINANCE
USSR STATE PLANNING COMMITTEE (GOSPLAN)
National budget priorities are established at the central (USSR) level in the Ministry of Finance and Council of Ministers
REPUBLIC COUNCIL OF MINISTERS
A court of appeal for city executive committee and gorfo. Provides general policy guidance for Ministry of Finance and republic Gosplan.
REPUBLIC MINISTRY OF FINANCE
REPUBLIC STATE PLANNING COMMITTEE (GOSPLAN)
Consult with each other about plan and budget.
City executive committee may, together with gorfo, appeal budget restrictions to Republic Ministry of Finance.
Proposed city budget after approval by the city executive committee is sent to the Ministry of Finance. Gorfo and the Ministry of Finance work on the budget together.
MAYOR
CITY EXECUTIVE COMMITTEE
CITY SOVIET
GORFO (The Municipal Finance Department)
GORPLAN (The Municipal Planning Commission)
Gorplan participates closely with gorfo in the formation of the budget. Gorplan and gorfo meet frequently. Gorplan gives control indices to gorfo.
MUNICIPAL ADMINISTRATION, DEPARTMENTS, COMMISSIONS AND ORGANIZATIONS
Municipal administrations, etc. submit their prospective budgets to gorfo. Then they bargain informally and formally, directly with gorfo or through the city executive committee.
CITY DISTRICT FINANCE DEPARTMENTS
CITY DISTRICT PLANNING ORGANS

Fig. 1. Vertical-Horizontal State Relationships During Municipal Budget Formulation

cials to discuss these revisions. A final draft budget is produced and then submitted to the municipal executive committee in early fall. Municipal officials usually accept the final draft budget without reservations, although it is legally possible to appeal to the republic for additional funds. In the late fall, after all adjustments have been made, gorfo draws up its final version of the budget, in collaboration with gorplan and the municipal executive committee. The final budget is printed up and circulated to all municipal administrations, to city district executive committees and to other leading municipal officials. Changes made at this stage are minor. The final budget is formally submitted by the executive committee to the city Soviet, for ratification by the deputies at their December session. (Copies of the budget are distributed to the city Soviet's standing committee on budget and finance several weeks in advance of the session. The committee has the power to recommend changes but rarely does so.) At the session, the budget is approved unanimously, usually without much discussion. The budget now becomes a legal document, binding upon all municipal organs. It is then officially published, and the new budgetary cycle for the following year begins.

The above description of budget formulation shows that the republic level exercises strong control over municipal finance. The control figures that gorfo uses come from the republic: the municipality, having almost no revenues of its own, is totally dependent upon revenues allocated to it from above; the republic can make arbitrary budget cuts, from which the municipality has little recourse; municipalities secure few real gains in negotiations with republic financial authorities. It is true that gorfo speaks for the municipality during the process of budget formulation; but gorfo is also the municipal arm of the republic Ministry of Finance, so its municipal loyalties are questionable, despite its formal subordination to the municipal executive committee.

On the municipal level, gorfo and the executive committee share responsibility for budget formulation. Gorfo provides information about control indices and the technical details of budgeting. Within this framework, the municipal executive committee coordinates the claims and demands of the various municipal administrations. How closely do gorfo and the executive committee cooperate? According to some municipal officials, there is minimal cooperation between the two organs, even though the head of gorfo usually sits on the executive committee. One gorfo official noted,

> "No one likes us very much. The municipal administrations know that we are going to limit their expenses, and other members of the executive committee claim that we are not committed enough to the municipality. Then, too, few other municipal officials have the technical expertise which is needed to cope with detailed budget and financial matters."

How well does the executive committee get along with the municipal administrations during the budgetary process? Since the heads of major municipal administrations also sit on the executive committee, and since there rarely are large uncommitted funds to argue over, relations between the executive committee and the administrations are fairly routine. The mayor, the heads of the key administrations and one or two other leading municipal officials usually resolve any conflicts while the budget is in executive committee.

The ordinary deputy to the city Soviet hardly participates in the budgetary process. He is called upon only at the end, to ratify a document which is considered an established fact by municipal officials. The average deputy, like aldermen in most North American cities, cannot debate budgetary provisions, many of which are too complex for him to understand. He has precious little time to read the text of the budget before its adoption at the December session. A few deputies are members of the city Soviet's standing committee on budget and finance, but this committee has only recommendatory powers. "Just as in your country," noted one Leningrad official, "the average city deputy knows very little about the budget. He has neither the time nor the expertise to understand the document in its entirety. But he will be able to understand the budget's basic provisions."

What role does the Party perform during budgetary formulation? The Party (whose members comprise about five per cent of the Soviet population) establishes basic priorities and guidelines for officials in the Ministry of Finance, and then, apparently, leaves the budgetary process in state hands except for emergencies (a sudden revision of economic priorities or a dispute among state agencies or levels). Several officials pointed out that the Ministry of Finance is relatively free from Party control and politics, noting that the Ministry developed a tradition of expertise and independence under the leadership of A. G. Zverev.[9] Top financial officials are, however, Party members; each of the twelve financial officials whom I interviewed were Party members, although none of them had worked in the Party apparatus for the last ten years. The Party also exerts its controls on the municipal level in a number of ways. First, the top officials in the city Party organization (the first secretary and other Party secretaries) are members of the municipal executive committee; they represent the Party during budget discussions. Second, every key member of the municipal executive committee is a Party member. The heads and top staff of most municipal administrations are also Party members.[10] Third, the city Party organization has its own financial and budget specialist who can give advice and information to municipal officials. Fourth, the city Party helps to train municipal deputies, to explain the basic features of the budget to them, and to show them how to communicate this information to their constitutents. Finally, the Party functions as a supercoordinator, a municipal arbiter, and provides a constant check upon the work of state organs. These Party controls may not be as obvious during the budgetary process as they are during the formulation and implementation of other policies within the municipality, but they are available to the Party should it choose to exercise them.

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[9] Arseny G. Zverev was USSR Minister of Finance from 1938 to his retirement in 1960, except for a brief ten-month period in 1948, when Alexei Kosygin held the post. Zverev's 22-year tenure as head of the USSR Ministry of Finance was an extraordinary achievement.

The Planning Process

The budget is actually one part of the annual city plan, which effectively controls the activities of all individuals, enterprises and organizations under municipal jurisdiction. Everything, everybody, has his plan—an officially determined set of productivity expectations. The city plan is a mosaic of many smaller plans, stuck together in the hope of providing maximum productivity and efficiency. The process of making and approving a city's annual plan follows the same basic pattern as does budget formulation. Gorplan, the municipal arm of the republic State Planning Committee, is the municipal equivalent of gorfo. Republic state and Party organs exercise strong control over the work of gorplan. For example, in the city of Baku, "the annual city plan is based upon control figures worked out by various Azerbaidzhan republic ministries, together with Azerbaidzhan Gosplan (the Azerbaidzhan republic State Planning Comittee), and affirmed by the Central Committee of the Azerbaidzhan Communist Party and the Council of Ministers of the Azerbaidzhan republic." The Baku city plan is worked out, "based on these republic directives and the needs of the Baku city Soviet."[11] Thus Baku, a major Soviet city, has little control over its own annual plan, and the Baku city Soviet must work within a framework which is established by republic Party and state organs and which is then communicated to the municipality through gorplan.

Once it has received its instructions from above, gorplan consults with the executive committee and then sends the control figures for the new plan to all municipal administrations. The latter submit draft plans to gorplan in late spring. Gorplan then produces a consolidated municipal draft plan, confers with the executive committee, making some additional changes, and then sends the municipal draft to Gosplan in June. Republic authorities make extensive changes, usually revising productivity targets upward, reducing operating expenses, and restructuring municipal priorities to conform to last minute Party instructions. The draft plan now returns to the municipality, and the executive committee and the municipal administrations examine the revised draft, hoping to restore some of the fat which republic authorities have trimmed off. The municipality lobbies with its republic superiors for additional concessions, such as a greater allotment for housing, and a reduction in municipal productivity targets. In November, copies of the final plan are circulated to top municipal officials prior to the December session of the city Soviet, at which the plan and budget are formally ratified, with little discussion and no reservations.

While planning and budget formulation are closely intertwined and both processes follow similar patterns, the Party is comparatively more active during the planning process. There is more bargaining and conflict among municipal and republic organs, and municipal organs now have a moderate amount of autonomy in certain areas of the planning process. One Leningrad official observed, "The budget is a technical document, and the Party, for the most part, leaves the budget to the financial experts. The plan is another matter. It represents the Party's allocation of priorities during the coming year, and is more of a political document, requiring greater Party control." The head of gorplan, for example, is often a former full-time

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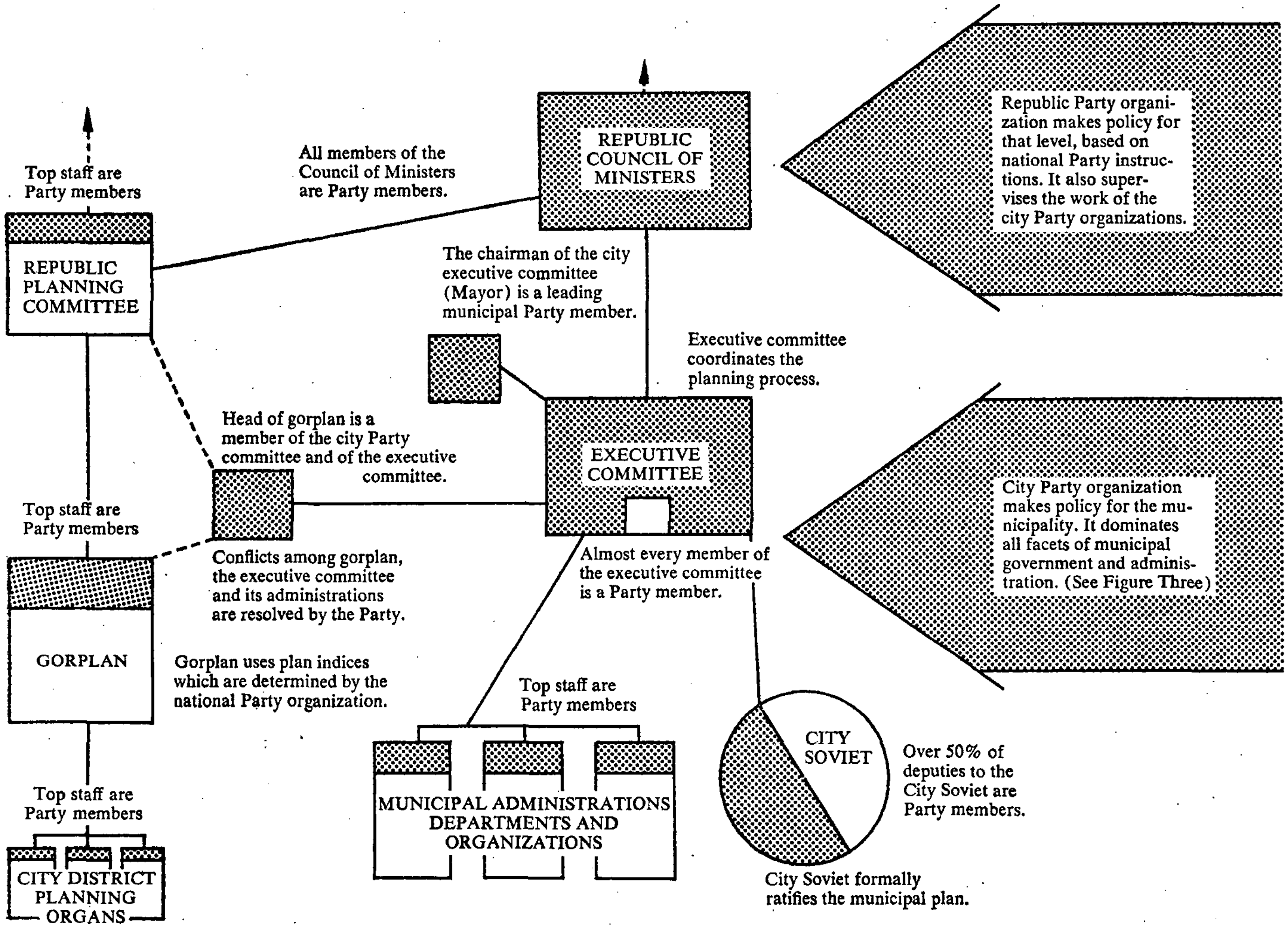
[10] When Mikhail Vasilevich Posokhin became head of Moscow's Administration of Architecture and Town Planning in 1961, he simultaneously joined the Party. All Moscow administration heads are Party members. In small cities, non-Party members head the less important municipal departments.

[11] "Zadachi i rabota bakinskoy gorodskoy planovoy komissii" [Tasks and Work of the Baku City Planning Commission], Baku, 1965, p. 1. This seventeen-page document was specially prepared for me by the Baku planning commission.


REPUBLIC COUNCIL OF MINISTERS
REPUBLIC PLANNING COMMITTEE
EXECUTIVE COMMITTEE
GORPLAN
CITY DISTRICT PLANNING ORGANS
MUNICIPAL ADMINISTRATIONS DEPARTMENTS AND ORGANIZATIONS
CITY SOVIET

Top staff are Party members

All members of the Council of Ministers are Party members.

Republic Party organization makes policy for that level, based on national Party instructions. It also supervises the work of the city Party organizations.

The chairman of the city executive committee (Mayor) is a leading municipal Party member.

Executive committee coordinates the planning process.

Head of gorplan is a member of the city Party committee and of the executive committee.

Top staff are Party members

City Party organization makes policy for the municipality. It dominates all facets of municipal government and administration. (See Figure Three)

Conflicts among gorplan, the executive committee and its administrations are resolved by the Party.

Almost every member of the executive committee is a Party member.

Gorplan uses plan indices which are determined by the national Party organization.

Top staff are Party members

Over 50% of deputies to the City Soviet are Party members.

Top staff are Party members

City Soviet formally ratifies the municipal plan.

Fig. 2. Party Dominance over the Governmental-Administrative Organs Engaged in the Municipal Planning Process
(Shading indicates Party membership or Party Candidate membership)

Party official. Planning specialists sitting on the municipal executive committee usually have had extensive Party experience.[12] Party organs directly intervene during the planning process to provide information, stimulation, and coordination. The Party has its own planning specialists—experts in trade, construction, light and heavy industry, transportation, and the municipal economy. These Party specialists frequently step in during the municipal planning process, giving the Party viewpoint on a particular matter, and offering guidelines and other advice to municipal officials.[13] The Party also plays an important role in the solution of conflicts which arise during the planning process. "The municipal executive committee is not always in a position to resolve these conflicts, and sometimes the Party bureau steps in to settle disputes," said a Tashkent city official. "For example, both the city and republic Party organizations had to intervene to resolve a recent jurisdictional dispute between two Tashkent administrations. The executive committee had been unable to settle the conflict, and the dispute had been hampering the planning process."

What is meant by "municipal autonomy" in the planning process? It is hard to conceive of "autonomy" when basic planning decisions are made by nonelected officials from a higher territorial administrative level. In the Soviet context, however, it is apparent that municipal organs have acquired a moderate amount of autonomy (perhaps "initiative" is more appropriate) in certain areas of the planning process, probably due to a number of historical and developmental factors. The death of Stalin, the decentralizing administrative reforms in 1957, rapid urbanization, consumer-oriented policies, an increase in the technical and educational qualifications of lower level officials—all these factors in varying degrees, have contributed to some democratization of the municipal decision-making process. How is this shown during the highly centralized municipal planning process? First, "the city Party organization has become more independent in its relationships with higher Party organizations, and is less inclined simply to carry out orders from above."[14] Second, municipal officials have acquired some competence in planning, and higher authorities are willing to entrust them with more responsibilities. Third, the scale of municipal government has greatly increased in the past decade, and higher authorities are forced to delegate an increasing number of tasks to municipal administrators. The head of one municipal administration noted that he had served for more than twenty years in the administration, "but only in the last few years, when the volume of our work increased radically, did our administration begin to perform meaningful tasks within the planning process. Now our preliminary figures are not treated so rudely by our superiors, they consult us more often, and they have stopped imposing arbitrary production quotas upon us." To a Western reader, such examples do not add up to democratization, municipal autonomy, or greater initiative, although in the Soviet setting these changes are fairly significant.

Housing Construction and Allocation Staffing

The average Soviet citizen is allocated 97 square feet of housing space. In large cities this figure may drop to 60–75 square feet of housing. The prospect of new and better housing, therefore, is a major preoccupation of most urban dwellers, and probably constitutes the most important municipal decision affecting their daily lives. Supply and maintenance of housing is becoming a major (though by no means exclusive) concern of Soviet municipalities. Cities have gained considerable responsibilities in this area in the past decade. Larger cities have begun to take over the allocation and administration of housing from factories and ministries which had in the past monopolized these functions and had not been subject to municipal jurisdiction. Municipal housing construction organizations have been established in the three largest Soviet cities, Moscow, Leningrad, and Kiev, and housing design has become more decentralized and responsive to the needs of particular urban environments. Even so, municipalities today have only partial control over urban housing. Higher level state and economic organizations are reluctant to relinquish control over this scarce commodity which is used as an inducement for attracting and keeping workers. Thus even in 1965, the city of Moscow still administered only two-thirds of the housing located on its territory; the rest was in the hands of some 300 organizations and enterprises not under the Moscow government's jurisdiction. Four years later, in 1969, a Moscow housing official said that the city was administering 75 per cent of the housing located on its territory,

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[12] This observation is based upon an analysis of the career patterns of members of Soviet municipal executive committees, 1957–1971, and from personal interviews.

[13] One municipal official observed that "Certain city Party officials are apparently born with a telephone permanently attached to their body. They are always calling, always checking, always complaining."

[14] From an interview with the first secretary of a city Party organization.

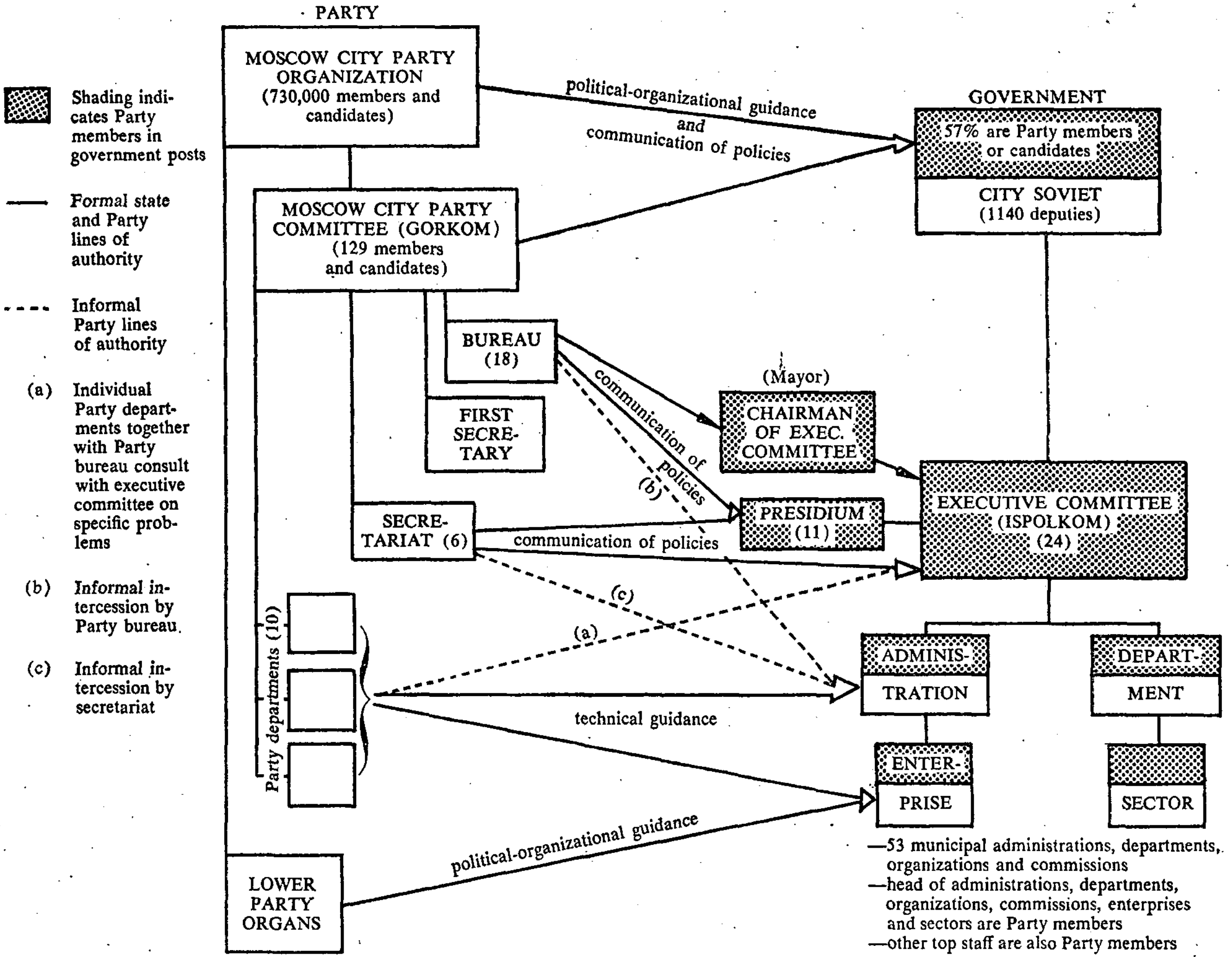
Shading indicates Party members in government posts
Formal state and Party lines of authority
Informal Party lines of authority
(a) Individual Party departments together with Party bureau consult with executive committee on specific problems
(b) Informal intercession by Party bureau.
(c) Informal intercession by secretariat
PARTY
MOSCOW CITY PARTY ORGANIZATION (730,000 members and candidates)
political-organizational guidance and communication of policies
GOVERNMENT
57% are Party members or candidates
CITY SOVIET (1140 deputies)
MOSCOW CITY PARTY COMMITTEE (GORKOM) (129 members and candidates)
BUREAU (18)
FIRST SECRETARY
communication of policies
(b)
(Mayor)
CHAIRMAN OF EXEC. COMMITTEE
PRESIDIUM (11)
EXECUTIVE COMMITTEE (ISPOLKOM) (24)
SECRETARIAT (6)
communication of policies
(c)
(a)
Party departments (10)
technical guidance
ADMINISTRATION
DEPARTMENT
ENTERPRISE
SECTOR
LOWER PARTY ORGANS
political-organizational guidance
—53 municipal administrations, departments, organizations and commissions
—head of administrations, departments, organizations, commissions, enterprises and sectors are Party members
—other top staff are also Party members

Fig. 3. Party Domination of Municipal Government in Moscow, 1966

"in keeping with a continuing national trend toward giving Soviet municipalities full control over housing."

Housing policy is formulated at the national level by the Party and communicated to republic and municipal Party and state officials in a fashion resembling budget formulation and planning.[15] Annual housing construction figures, allocation priorities and funds for housing maintenance and repair are included in the city's plan. A large number of municipal administrations are involved in various aspects of housing construction, design, allocation and administration, and many of the key members of the city Party bureau and the executive committee have had extensive experience in the field of housing.[16] Figure 4 shows how the various municipal administrations and the executive committee divide responsibilities for housing in Moscow. The Main Administration of Construction (Glavmosstroy) builds 85 per cent of Moscow's housing, has 250,000 employees and an annual payroll of $300 million. Glavmosstroy is responsible to the municipal executive committee and to Gosstroy, the State Construction Committee. The head of Glavmosstroy always is a member of the executive committee.[17] Glavmosstroy meets with the executive committee early in the year to discuss details of Glavmosstroy's forthcoming yearly plan and to coordinate housing construction with other phases of the housing "process." For example, the Main Administration of Town Planning and Architecture (Glavapu) designs the apartments and buildings which Glavmosstroy builds. These two administrations meet together to coordinate housing construction and design. Because the Main Administration of Construction Materials provides building supplies to Glavmosstroy, the executive committee tries to ensure that this administration's production schedule fits in with Glavmosstroy's construction schedule. Glavmosstroy cannot get paid by the state until another administration, the Main Administration of Capital Construction, has inspected the completed building and given its approval. At this point disputes often arise, and the executive committee may have to intervene. Once the building is legally completed, the Administration for the Allocation of Housing takes charge of the distribution of this new housing and the Main Administration of Housing assumes the responsibility for the operation of and repairs to Moscow's housing.[18]

Municipalities are faced with endless problems and crises during this housing cycle. Construction targets are almost never filled because of delays in getting materials, low worker productivity, construction mistakes and overly ambitious construction targets. There is always a chronic shortage of housing. The municipal waiting lists for new housing space are so long that citizens may have to wait several years to get new or better housing. It is almost as difficult to get housing repaired. The Main Administration of Housing just cannot meet an ever-increasing demand for urgent repairs to new and old housing, despite the creation of hundreds of decentralized housing repair units. Desperate tenants resort to black market repairs, paying a "bonus" out of their own pockets to "repair entrepreneurs," who make illegal repairs on state time, using state materials. The Soviet press continually condemns these practices, berates municipal officials for failing to improve housing and exhorts construction workers to improve the speed and quality of their work. Conscious of the impact which housing has on the average urban dweller, the Party encourages public criticism of housing inadequacies, thereby hoping to improve the performance of individuals and officials involved

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[15] Current Soviet housing policy is based upon the July, 1957, resolution of the Central Committee of the Communist Party, "On the Development of Housing Construction in the USSR." This resolution established a number of important housing principles, such as "massive industrialization of housing construction" and "greater control over housing by municipal executive committees."

[16] The last three mayors of Moscow had long careers in housing construction and administration. N. I. Bobrovnikov, who was mayor from 1956–1961, spent most of his career as a Moscow housing expert; N. A. Dygai (1961–63) was USSR Minister of Construction prior to his appointment as mayor; V. F. Promyslov, the current Moscow mayor, was Minister of Construction for the Russian Republic.

[17] V. F. Promyslov, the first head of Glavmosstroy, later became mayor of Moscow; A. A. Etmekdzhiyan, a deputy head of Glavmosstroy, became a top USSR State Construction Committee (Gosstroy) official; N. Pashchenko, Promyslov's successor as head of Glavmosstroy, now holds a high Russian Republic state post.

[18] State housing is allocated on a space per capita basis, with 97 square feet representing the "norm." Certain individuals and organizations have priority over others in the allocation of space. Writers and other professionals frequently can obtain an extra room; military organizations get preference over civilian organizations. These and other priorities are usually regulated by statute. See "Ob utverzhdenii polozheniya, 'O poryadke raspredeleniya zhiloy ploshchadi v Moskve'" [On Affirming the Statute, "On How Housing Space Will Be Distributed in Moscow"], Decision of the Moscow city executive committee, May 4, 1965, *Byulleten ispolnitelnovo komiteta moskovskovo gorodskovo Soveta deputatov trudyashchikhsya* (Bulletin of the Executive Committee of the Moscow City Soviet), 10, (1965), 22–27. This municipal statute is a duplicate of the republic housing statute.

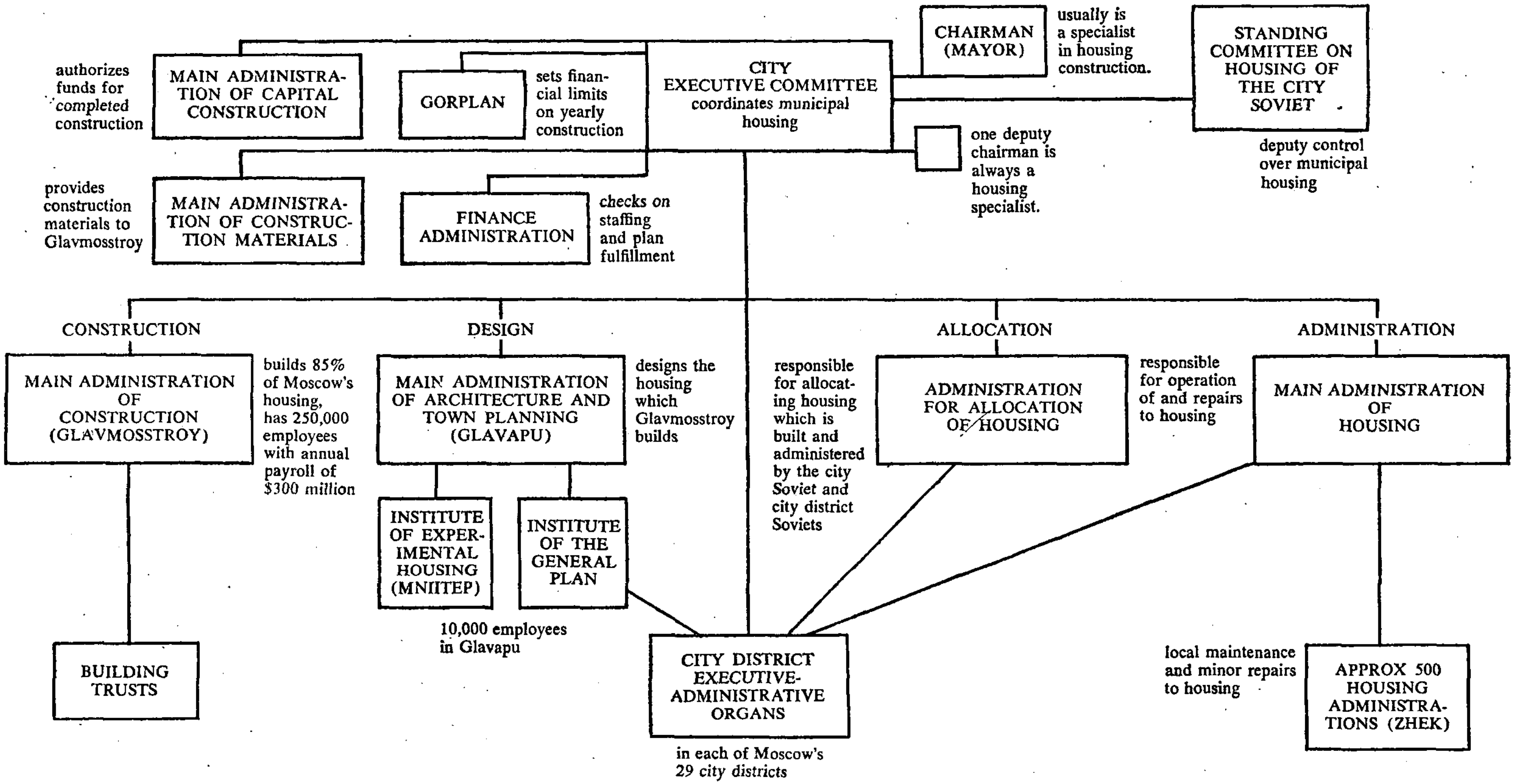
authorizes funds for completed construction
MAIN ADMINISTRATION OF CAPITAL CONSTRUCTION
GORPLAN
sets financial limits on yearly construction
CITY EXECUTIVE COMMITTEE coordinates municipal housing
CHAIRMAN (MAYOR)
usually is a specialist in housing construction.
STANDING COMMITTEE ON HOUSING OF THE CITY SOVIET
one deputy chairman is always a housing specialist.
deputy control over municipal housing
provides construction materials to Glavmosstroy
MAIN ADMINISTRATION OF CONSTRUCTION MATERIALS
FINANCE ADMINISTRATION
checks on staffing and plan fulfillment
CONSTRUCTION
DESIGN
ALLOCATION
ADMINISTRATION
MAIN ADMINISTRATION OF CONSTRUCTION (GLAVMOSSTROY)
builds 85% of Moscow's housing, has 250,000 employees with annual payroll of $300 million
MAIN ADMINISTRATION OF ARCHITECTURE AND TOWN PLANNING (GLAVAPU)
designs the housing which Glavmosstroy builds
responsible for allocating housing which is built and administered by the city Soviet and city district Soviets
ADMINISTRATION FOR ALLOCATION OF HOUSING
responsible for operation of and repairs to housing
MAIN ADMINISTRATION OF HOUSING
INSTITUTE OF EXPERIMENTAL HOUSING (MNIITEP)
INSTITUTE OF THE GENERAL PLAN
10,000 employees in Glavapu
BUILDING TRUSTS
CITY DISTRICT EXECUTIVE-ADMINISTRATIVE ORGANS
in each of Moscow's 29 city districts
local maintenance and minor repairs to housing
APPROX 500 HOUSING ADMINISTRATIONS (ZHEK)

Fig. 4. Housing Construction, Design, Allocation and Administration in Moscow, 1969

in the housing cycle and to provide an outlet for citizen discontent with poor housing. During municipal election campaigns, prospective candidates are most frequently questioned about housing matters. I attended several municipal "campaign speeches" in Moscow and in Leningrad, and housing always was the main topic of discussion.

The city Party organization devotes a great deal of its energies to municipal housing problems, particularly in cities which have their own housing construction organizations. One city Party secretary always is a housing specialist, and two or three Party departments (usually in construction, industry, and the municipal economy) concentrate on housing problems. According to one city Party secretary, "More than half the decisions made by the Party bureau (the most important city Party organ) have to do with housing. The Party bureau establishes municipal housing policy, in cooperation with the municipal executive committee." Since the key members of the executive committee (the Party secretaries, the mayor) sit on the Party bureau, it is clearly the Party bureau which makes any decisions affecting housing policy in the municipality.[19] Besides its responsibility for policy formulation, the city Party organization provides political, organizational and technical guidance to municipal housing administrations. By means of campaigns in the media, and through its control over the trade unions and youth organizations, the Party constantly tries to improve the pace and quality of municipal housing construction. The city Party committee organizes all Party members within a given city organization into Party groups which are to meet regularly to receive and to discuss Party instructions. Municipal housing officials may find themselves asking Party housing specialists for technical assistance, advice about policy implementation, and help in coordinating their work with other municipal agencies. At the lower levels, the Party explains housing policies to citizens and hears their complaints, either through specially trained Party activists in local housing administrations, at work, or in public organizations; or through municipal deputies, who are trained and supervised by the city Party organization. From top to bottom, therefore, from mayor to ordinary citizen, the Party is involved in the municipal housing cycle, making housing policy and then presiding over its implementation.

Staffing

In the USSR, the Communist Party traditionally is responsible for "the selection and appointment of leading personnel, their education in the spirit of communist ideas, honesty and truthfulness, and a high sense of responsibility to the Party and to the people for the work entrusted to them." The Party ensures "the appointment of the best leaders in the most responsible areas of the state apparatus."[20] On the municipal level, all top posts, Party or non-Party, are staffed by the Communist Party with Party members. If a municipal post is important, it is usually held by a Party member, who, of course, has been appointed by the Party.[21] Within each municipality there are three levels of staffing jurisdiction (*nomenklatura*). Top non-Party posts are staffed by the city Party organization on the recommendation of higher Party and state authorities. Middle range non-Party posts normally are staffed solely by the city Party organization. Lower level non-Party posts are filled by municipal non-Party bodies.

Top municipal non-Party posts—such as the mayor, deputy chairmen of the executive committee, heads of municipal administrations, and the head of the city trade unions—are staffed by the city Party organization on the recommendation of higher Party and state bodies. What does this mean in actual practice? Does the city Party organization have any initiative here, or does it merely serve as a rubber stamp for staffing decisions made on a higher level? These questions are difficult to answer, because Soviet authorities do not publish many details about staffing procedures, particularly when Party organizations are directly involved. It appears that city Party staffing initiative varies in intensity and scope, depending upon the type of post, the size of the city and the personalities of those involved. According to the first Party secretary of a small city of 150,000 inhabitants, "most top non-Party posts are generally staffed from above. The city Party organization may recommend names, but the actual staffing is done from above." Party officials in large cities said that the city Party organization has more initiative than that in staffing top non-Party posts, particularly in the appointment of deputy chairmen of the executive committee. In large cities, the city Party organization probably participates more closely in the appointment of heads of municipal administrations. In Moscow, "the head of the Main Administration is nominated by the republic State Construction Committee after consultation with the city Party organization,"[22] while several Leningrad administration heads referred to greater city Party and state control over the appointment of heads of Leningrad administrations. Yet these are relatively minor changes, and, on balance, they do not indicate major gains in staffing autonomy. The city Party organization still continues to be dominated by higher authorities in the staffing of top municipal non-Party posts.

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[19] For example, two weeks after the Party Central Committee passed its July, 1957, resolution "On the Development of Housing Construction in the USSR," the Moscow city Party bureau met to implement this resolution in Moscow. The bureau "affirmed the July resolution" and promised "to emphasize these policies in Moscow." It "approved the present work of the city Soviet's Main Administration of Construction" and instructed its members "to show municipal organizations how to carry out the July resolution." See *Nekotorye voprosy organizatsionno-partiinoy raboty v sovremennykh usloviyakh* [Several Questions of Organizational-Party Work in the Present Time] (Moscow: Higher Party School and Academy of Social Sciences of the Central Committee of the CPSU, 1961); and A. A. Levsky, "Deyatelnost moskovskoy organizatsii KPSS po vypolneniyu reshenii XX syezda partii v oblasti uluchsheniya zhilishchnykh uslovii trudyashchikhsya" [The Work of the Moscow Party Organization in Fulfilling the Decisions of the 20th Party Congress Concerning the Improvement of Housing for Workers], dissertation for candidate degree in history, Moscow, AON pri TsK KPSS, kafedra of Party history, 1961.

[20] *The Statutes of the Communist Party of the Soviet Union, 1961.*

[21] This section does not discuss the staffing of top city Party posts (Party secretaries, members of the Party bureau, Party department heads). These posts are normally staffed by higher Party organizations.

Middle range non-Party posts are staffed exclusively by the city Party organization. What kinds of posts are these, and do they present distinctive staffing patterns? The range of posts is wide, comprising factory directors, heads of research institutes, key media personnel, the chairmen and deputy chairmen of city district executive committees and leading cadres in the municipal executive committee and its administrations. Following are examples of middle range staffing in Leningrad and Riga:

> In the past two years the Leningrad Party committee has placed a large number of Party workers in Soviet trade union work. . . . Two former city Party department heads are now chairmen of city district Soviets. The former first secretary of the Lenin city district, Yu. M. Solomonov, is now deputy chairman of the Leningrad city Soviet.
>
> The Riga city Party committee transferred 42 people in its *nomenklatura* to other posts. Comrade Bobrov, secretary of the Kirov city district Party committee, was demoted and made director of a tobacco factory; . . . the head of the industrial-transport department of a city district Party committee was demoted to the post of deputy director for cadres in a factory; the first secretary of the Moscow city district Komsomol (Party youth) organization was transferred to an economic post.[23]

Of 660 posts staffed by the Riga Party organization in 1966, 253 (38.3 per cent) were "workers in Party and Soviet organs"; 107 (16.2 per cent) were "economic officials" (factory directors and top staff); 300 (45.4 per cent) were divided among "ideological organizations," "educational organizations," "administrative and trade-financial organs," and "workers in construction and the municipal economy."[24] The most distinctive staffing pattern on this level, according to municipal officials, is a noticeable increase in the quality of these appointments and a greater willingness on the part of city Party officials to consult with relevant non-Party organs before staffing posts which require specialized skills.

The Party staffs the top and middle range municipal posts, but non-Party agencies fill a large number of lower-level posts. A Western study estimates that 80–85 per cent of Soviet non-Party posts "are completely within the purview of the employing agency, subject to compliance with existing classification schedules, tables of organization and service regulations."[25] Barghoorn notes that the Ministry of Finance "determines the wage fund of ministries, state committees and enterprises" and thus is a key participant in the staffing process, since it must approve increases or decreases in the wage funds of state organs. The State Staffing Commission (*Gosudarstvennaya shtabnaya komissiya*) is instrumental in setting up salary schedules, job descriptions and tables of organization; the state Committee on Labor and Wages "works out uniform salary scales" and "legislation on salaries and wages."[26] Municipal executive committees have their own *nomenklatura*, and can appoint "cadres of the city Soviet."[27] The municipal organizational-instruction department, which trains and organizes municipal deputies, is called upon by the executive committee during the staffing of its *nomenklatura*, to assess the performance and capabilities of prospective appointees. According to several municipal officials, the executive committee's *nomenklatura* is growing in size and in importance, in keeping with the expanding governmental-administrative responsibilities of Soviet municipalities. Municipal branch administrations now have larger staffs to be appointed, and their vertical superiors are becoming less inclined to interfere in municipal staffing procedure.

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[22] From an interview with a Glavmosstroy official.

[23] See N. N. Popov, "Podbor, rasstanovka i vospitaniye kadrov" [The Selection, Placement and Training of Cadres], *Voprosy partiinoyo stroitelstva* [Questions of Party Construction], (Leningrad: Lenizdat, 1962), p. 339; and Yu. Ruben, "Rabota s kadrami—produmannuyu sistemu" [Working with Cadres—A Well Thought Out System], *Kommunist*, 1 (1966), 36–37.

[24] Yu. Ruben, "Rabota s kadrami—produmannuyu sistemu," p. 36. Riga's *nomenklatura* is comparatively smaller than some other municipal staffing lists (In 1958, for example, Moscow had a *nomenklatura* of 17,000). The Riga posts are probably those posts staffed by the central city Party organization and exclude a large number of lesser posts staffed by city district Party organizations.

[25] *Staffing Procedures and Problems in the Soviet Union* (Washington, D.C.: US Government Printing Office, 1963), p. 17.

[26] Frederick C. Barghoorn, *Politics in the USSR* (Boston: Little, Brown and Company, 1966), pp. 290–291. See also Merle Fainsod, *How Russia is Ruled* (Cambridge: Harvard University Press, revised edition, 1963), pp. 414–417.

Is there a trend toward reduction in Party dominance over the staffing of municipal non-Party posts? With Soviet municipalities now performing expanded, more complex governmental-administrative tasks, and with a continually growing urban population, the absolute number of non-Party posts within municipalities has risen and the Party just cannot handle the staffing of all these posts. The criteria for staffing are also shifting in favor of merit principles. Political reliability may be becoming less of an issue, with technical-rational expertise becoming the pertinent factor. "Getting the job done" now demands highly specialized knowledge of construction and engineering techniques, as well as traditional political-organizational skills. Can the Party become specialized enough to be able to use technical-rational criteria effectively in staffing, or will it have to delegate its authority to those non-Party bodies which have this competence? Several points speak out against a decline in the Party's dominance over the staffing of municipal non-Party posts. First, the Party is itself changing and is acquiring more specialists who are rationally-technically oriented. Thus it may not have to rely upon non-Party bodies to supply this sort of expertise. Second, the Party fully controls the instruments for punishing and firing officials whom it deems to be incompetent. Even if the Party cannot appoint everybody, it keeps a close check on everybody's performance. Third, the Party will always appoint the top municipal officials—the political leaders—and thus is in a position always to influence other appointments. Finally, even if non-Party bodies appoint someone to a municipal post, why should this automatically produce a decline in the Party's role in the municipality? The prospective appointee almost certainly is a Party member, proposed by the leading officials (who are, of course, Party members) in a non-Party body. Actually the real issue confronting municipal governments may not even be Party dominance. For many municipal officials the real issue is too much higher level interference, from *both* state and Party bodies. Even the city Party organization is not permitted to staff its own top posts. But it is unlikely that these vertical controls will be lifted in the near future.

Soviet Municipal Government, Present and Future

The foregoing analysis of the Soviet municipal decision-making process in budget formulation, planning, housing and staffing has emphasized three basic features. First, the Communist Party stands squarely in the center of this process, making all the basic policies, appointing key municipal officials to implement these policies, and serving as the "boss" of the municipality. Second, higher level authorities decisively and consistently interfere in the municipal decision-making process. The budget and plan are handed down from above. The municipality is, in effect, told what to produce and how its resources are to be distributed. The city's top leaders, state and Party, are imposed from above and can be removed at the pleasure of higher authorities. Compared with Western municipalities, Soviet cities are so dominated by higher level authorities that one wonders if it is even appropriate to speak of a Soviet municipal decision-making process, since decisions allocating municipal priorities have already been made elsewhere, in higher state and Party bodies. Yet, ironically, the third feature of Soviet municipal decision making is a slight trend toward greater initiative and autonomy for Soviet municipalities in their self-government. This is evident in housing, in which some municipalities have expanded their rights, and in planning and staffing, where cities are making some gains. Admittedly these gains are small, and they occur mainly in the larger Soviet cities, but when one remembers that the Stalinist days of hyper-centralization still existed less than a generation ago, then these gains in muicipal autonomy, small as they may seem to Vestern readers, are significant.[28]

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[27] L. Karapetyan and V. Razin, *Sovety obshchenarodnovo gosudarstva* [The Soviets of the State of All the People] (Moscow: Politicheskava literatura, 1964), pp. 103–104. Fainsod observed that in Smolensk a large number of appointments were made by the Soviet, which had its own *nomenklatura* for "junior appointments" and "technical specialists." "Leading workers," on the other hand, were appointed by the Party (*Smolensk Under Soviet Rule*, p. 100).

In the future, decision making in Soviet cities ill be influenced by three already noticeable endencies. First, the nature and quality of arty control is changing. The educational ualifications and professional expertise of uran Party members are rapidly rising and will ontinue to do so. An analysis of the career atterns and educational backgrounds of 59 unicipal Party officials holding an urban post etween 1958–1966 shows that 91.5 per cent ad a higher specialized education and only 5 er cent had a Party political education.[29] hese findings seem to confirm that municipal arty officials can be simultaneously red and xpert and can possess what Fischer calls "dual eadership skills," which combine political and echnical training.[30] Thus the Party, by becomng more specialized and rational-technically riented, would be able to communicate more ffectively with municipal administrators and to ontrol them. Second, municipal administrators re becoming more professionalized. Despite a idespread campaign between 1959–1964 to eprofessionalize administration at the lower evels, the number and quality of full-time muicipal administrators actually increased subtantially, the average tenure in office rose by 0 per cent in this period.[31] Crossover between arty and non-Party posts has decreased, and fficials are becoming now more committed to ong term careers which require extra specialzation and early career orientations. These paterns of increased specialization, reduced crossver from Party posts and longer tenure in urban administrative posts[32] are more pronounced in large cities—the most advanced Soviet sector and the model for future development. Finally, more attention is now being paid to the views and complaints of citizens in Soviet cities. *Ad hoc* citizen groups have managed, on occasion, to stop the razing of an historic church by impatient road builders; to publicize the problems of industrial pollution in a number of cities; to get transportation facilities expanded and improved in new urban residential areas. But the Party does not like to see the growth of such groups even if they are only *ad hoc* groups. Instead, citizens are still encouraged to present their grievances in institutionalized meetings with local deputies, by writing letters to *Pravda* and *Izvestia*, and through ritual citizen participation in thousands of Party-controlled public organizations, from apartment house committees to volunteer tree-planting brigades.

The study of Soviet municipal decision making can produce a fresh perspective on the way in which the Soviet political system functions. Since we rarely get any details about how decisions are made at the national level in the USSR, we must generalize from what occurs at the local level when we are fortunate enough to gain this information. Even partial information is welcomed, and it is tempting to make massive generalizations about the nature of Soviet politics when fresh data about aspects of Soviet life and politics are uncovered. I have tried to avoid such generalizations, but the reader is free to draw his own conclusions about how municipal decision making correlates with Soviet national decision making. Certainly it is likely that the same tendencies will occur in the USSR at both the local and the national levels especially in view of the hierarchial and totalist nature of the Soviet political system. The tendencies which I have noticed developing on the municipal level have been seen emerging on the provincial, republic and national levels by other scholars. Nevertheless, one ought to be cautious in making broader generalizations, at least until we have more data for a longer time period.

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[28] To clarify the expanding rights and responsibilities f Soviet municipalities, the Central Committee of the ommunist Party recently passed a major resolution hich, "strengthened the material and financial base f city governments" by increasing the sources of unicipal revenues. The resolution called for a major ncrease in municipal autonomy, "so that cities can eal effectively with current problems." So far, howver, it appears that the resolution has not led to ny significant restructuring of municipal rights and uties. See, "O merakh po dalneyshemu uluchsheniyu aboty rayonnykh i gorodskikh Sovetov deputatov udyashchikhsya" [On Measures for the Further Imrovement of the Work of the District and City Soets], *Pravda*, March 14, 1971 (Translated in *Current igest of the Soviet Press*, XXIII, April, 13, 1971, -5).

[29] See Frolic, "Some Soviet Elite Comparisons," *Candian Slavonic Papers*, 12 (December, 1970).

[30] George Fischer, *The Soviet System and Modern ociety* (New York: Atherton, 1968), p. 14.

[31] From a sample of 44 heads of municipal admintrations, departments, commissions, and organizaons.

[32] In Moscow and Leningrad between 1956–1965, the average tenure in office of heads of municipal administrations was approximately six years—twice as long as the average tenure of city Party committee members in both cities during that period. The comparison is not ideal, since city Party committee members perform other full-time jobs as well as holding city Party committee membership, but despite this reservation, municipal administrators clearly serve for much longer terms than most city Party officials.

Can we identify and compare distinctive Soviet and North American urban decision-making models? I think we can, eventually, although the comparisons will take time to develop, and again I want to introduce a note of caution. Because we lack data, it would be best to focus upon particular aspects of decision making, rather than upon the entire process. Municipal issue areas, such as housing and transportation, can be researched using Soviet materials, and we can produce urban elite comparisons from Soviet biographical data. Western scholars may find it necessary, however, to redefine "politics" and "decision making" in order to pursue these comparisons. Our notion of "conflict" poses problems for Soviet scholars and officials. One Soviet political scientist re marked to me recently, "You Westerners ar obsessed with conflict and consequently yo seem to have trouble in understanding our sys tem. You insist that conflict exists, is good, an has to be resolved publicly. In our system, how ever, because of our class viewpoint and the ex istence of the Communist Party, our bases fo disagreement are not the same, and we resolv our disagreements in a different manner." On can only hope that as more Soviet data abou decision making are made available to Wester scholars, and as we then improve our compara tive methodology and conceptualization, w may be able to affirm or refute the validity c his remarks about Soviet decision making.

nterest Groups and Political Integration: British Entry into Europe

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I. Introduction

This paper[1] analyzes the influence of interest roups in the formation of British policy oward successive European unity developments in the years from 1956 to 1967. Coniderable attention has been given elsewhere to he subject of Britain and Europe, particularly hrough detailed accounts of the various negoiations and the development of official views,[2] ut this study seeks to analyze the effect of he major interest groups upon the development of British policy toward Europe in order to test pecific hypotheses regarding the relationship between interest groups and the process of political integration.[3]

Two important bodies of writing and theorizing can be brought to bear in analyzing the relationship between group behavior and political integration. One of these is the group politics approach, which is set out in a coherent body of interest group theorizing based on British and American politics.[4] The hypothesis that may be inferred from this approach will here be termed the *group politics hypothesis.* The second major approach is that of functionalism; it figures predominantly in much of the recent work on political integration. In this paper the functionalist approach is extended beyond the area to which it has hitherto been applied, and this extension gives rise to an alternative *functionalist hypothesis.* The following sections will set out the context of the two hypotheses, test them against the empirical evidence derived from the British experience, and then offer some conclusions regarding the theoretical implications of these results.

II. Collectivist Politics in Britain and the Group Politics Hypothesis

This study is placed in the context of the British political process, which Samuel Beer has aptly described as one of "collectivist politics."[5] It differentiates two channels through which society and government interact. On the one hand there is the system of *party government*. Development of the welfare state and the realities of winning power have required that the political parties (each influenced by its distinctive conception of the common good) bid for the votes of consumer groups (blocs of voters), who thereby exercise their influence through the electoral process. The alternative channel linking society and government is that of *functional representation*.[6] Here, as development of the managed economy has involved the government in extending its control over the economy, the realities of governing in a free society have compelled the government to bargain for the active cooperation of the major interests involved.[7] Pressure groups embody these interests, and at least one prominent political scientist in Britain finds that they have become "a far more important channel of communication than parties for the transmission of ideas from the mass of the citizenry to their rulers."[8] Similarly, Harry Eckstein identifies a fading of major class and ideological contours and sees the emergence of a fundamental consensus on general policy matters which relegates conflict to points of detail. These trends lessen the intensity and importance of party competition and enhance the role of pressure groups in the political process.[9]

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[1] The study is based on data drawn from pressure roup materials and official publications, and from ixty elite interviews with civil servants, former cabinet ministers. party leaders, and officials of the major pressure groups. The major part of the interviewing was conducted in London during the spring of 1967. ubsequent work took place in Oxford and London uring 1969–70. For support, I wish to express my ppreciation to Harvard University for a Knox Traveling Fellowship, to the Social Science Research Council or a postdoctoral Research Training Fellowship, and o the University of California for a Faculty Research Grant.

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[2] See, for example, Miriam Camps, *Britain and the European Community, 1955–63* (Princeton: Princeton University Press, 1964), and her subsequent work, *European Unification in the Sixties: From the Veto o the Crisis* (New York: McGraw-Hill, 1966); Nora Beloff, *The General Says No: Britain's Exclusion From Europe* (Baltimore: Penguin, 1963); Uwe Kitzinger, *The Second Try: Labour and the EEC* (Oxford: Pergamon Press, 1968); Pierre Uri, *From Commonwealth o Common Market* (Harmondsworth, Middlesex: Penguin, 1968).

[3] This effort also differs from previous British pressure group case studies in that it examines neither an mportant piece of legislation nor the administration of an existing program, but rather the formulation of a policy, and one which is concerned with foreign ather than domestic matters. Cf. James B. Christoph, *Capital Punishment and British Politics: The British Movement to Abolish the Death Penalty, 1945–57* Chicago: University of Chicago Press, 1962); H. H. Wilson, *Pressure Group: The Campaign for Commercial Television in England* (New Brunswick, New Jersey: Rutgers University Press, 1961); and Harry Eckstein, *Pressure Group Politics: The Case of the British Medical Association* (Stanford: Stanford University Press, 1960).

[4] See, for example, Samuel H. Beer, *British Politics in the Collectivist Age* (New York: Knopf, 1965); S. E. Finer, *Anonymous Empire,* 2nd ed., rev. (London: Pall Mall Press, 1966); and Eckstein, *Pressure Group Politics.* For the United States, see also Raymond A. Bauer, Ithiel de Sola Pool, and Lewis A. Dexter, *American Business and Public Policy: The Politics of Foreign Trade* (New York: Atherton, 1963); and Lester W. Milbrath, "Interest Groups and Foreign Policy," in *Domestic Sources of Foreign Policy,* ed. James N. Rosenau (New York: Free Press, 1967), pp. 231–261.

[5] Beer, *British Politics in the Collectivist Age,* (see especially Chapter 12). Collectivist or corporatist politics are not confined to Britain: Joseph La Palombara describes in Italy, "a vast network of quasi-corporative relationships between certain interest groups and the administrative agencies." See La Palombara, "The Utility and Limitations of Interest Group Theory in Non-American Field Situations," in *Comparative Politics,* ed. Harry Eckstein and David E. Apter (Glencoe: Free Press, 1963), p. 427.

Several specific bases of group influence exist. To begin with, pressure group *advice* is essential in order for government to obtain basic information and technical knowledge without which economic regulation would be impossible. Next *acquiescence* is a necessity if governmental programs are to operate successfully. Finally, the *approval* of the groups concerned is required if particular governmental policies affecting them are to possess legitimacy.[10] This last consideration reflects a basic collectivist ethos which is shared by the Labour and Conservative Parties. In Labour's case it is expressed in terms of "Socialist Democracy," and for the Conservatives it is embodied in traditions of "Tory Democracy." The collectivist theory of representation, which sets both parties apart from the nineteenth-century political individualism of the Liberals, is a intrinsic part of twentieth-century British pc litical culture.

In the present context the term "pressur group" is understood to follow the relativel straightforward conception of S. E. Finer.] designates those organizations which are "oc cupied at any point of time in trying to ir fluence the policy of public bodies in their ow chosen direction; though (unlike politic parties) never themselves prepared to unde take the direct government of the country."[1] More specifically, this paper will concentrat on *sectional* pressure groups, those types c organizations whose political task it is to refle the interests of the economic or occupation sections they represent. Unlike promotion pressure groups, which are organized aroun attitudes and which generally seek to persuad people without regard to their sectional affili tion, the sectional groups do not direct thei attention to the parties, Parliament, or th electorate, which make up the party goverr ment sphere. Instead, as a result of goverr mental structure, activities, and attitudes, the normally concentrate their efforts upon th administrative departments of government.[1] Thus they typically operate through the functional representation process. Their bargainin power ultimately rests upon their performanc of crucial productive functions in the society and to the extent that government commit itself to intervene in the economy, it must obtain their cooperation. This need can be minimized in a totalitarian system, but in a countr with a system of representative governmen there is little choice but to secure a larg measure of voluntary cooperation from th bodies being regulated.[13] The actual mechanisms for group operation via functional representation are both formal and informal. The include numerous advisory committees oi which sectional groups are represented, as wel as close personal contact between representa tives of the interests and their opposite numbers in the Civil Service.[14]

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[6] As Beer defines it, the notion of functional representation is one which "finds the community divided into various strata, regards each of these strata as having a corporate unity, and holds that they ought to be represented in government" (p. 71).

[7] Beer, p. 321.

[8] R. T. McKenzie, "Parties, Pressure Groups and the British Political Process," *Political Quarterly*, 29 (January-March, 1958), 10.

[9] Eckstein, pp. 18–19.

[10] Beer, pp. 321–331.

[11] However Finer actually prefers the term "lobby to the term "pressure group." (See Finer, p. 3). In thi paper, the terms "pressure group" and "interest group will be employed interchangeably. There is howeve considerable—and occasionally tedious—treatment o the definitions elsewhere. See Gabriel Almond and G Bingham Powell Jr., *Comparative Politics: A Develop mental Approach* (Boston: Little, Brown, 1966), p 75; Allen Potter, *Organized Groups in British Nationa Politics* (London: Faber and Faber, 1961); Pete Self and Herbert J. Storing, *The State and the Farme* (Berkeley: University of California Press, 1963); an Wilson, Christoph, and Eckstein.

[12] Eckstein, pp. 16–17.

[13] Beer, p. 321.

While sectional pressure groups have a thoroughly economic basis, they do not function exclusively on the basis of a mechanistic economic self-interest for the following reasons: first, they seek to enhance their own legitimacy by identifying themselves with some conception of broader national interest which their actions are designed to advance. Thus the Confederation of British Industries (CBI, and its precursor, the Federation of British Industries, or FBI),[15] National Farmers Union (NFU), and Trades Union Congress (TUC)—which are the three most important groups and the ones treated in this paper—all articulate their demands above the level of crude advantage for businessmen, farmers, and trade unionists, respectively, even though their particular conceptions of free enterprise, agricultural prosperity, or working-class advancement still fall short of a more or less universalistic public interest. A second restraint upon these bodies is that their role depends on competence in specialized areas. The sectional pressure groups thus focus more upon matters of a technical nature than on overall policy. A third limitation is that the groups devote much of their attention to organizational self-maintenance—providing information and services and seeking to hold together a huge and heterogeneous membership with divergent interests. As a result of coping with internal stresses of this character, a group will often find it difficult to formulate a business, agricultural or labor view at the national level. Fourth, and finally, leadership constitutes a critical variable in the determination of group interest. A change in individuals can produce an entirely different organizational policy. In the present case, a nonactivist FBI President, a staunchly anti-European NFU President, and a pro-Common Market TUC leader were personally responsible for organizational viewpoints which differed sharply from those adopted under leaders who immediately preceded or followed them in office.[16]

Despite such restraints, these groups do occupy positions of great power within the British political system. How, then, can the amount of their power—particularly in specific policy matters—be assessed? In dealing with the Italian experience, Joseph La Palombara judged it impossible to measure the influence of groups over administrative decisions.[17] It is possible, however, to attempt such a judgment by utilizing the characterization of the pressure group role put forward by S. E. Finer. He finds that although organized capital and labor in Britain do not dictate public policy, their position in the economy makes it essential that their cooperation be won. Essentially, the relationship is such that "they do not direct but they may veto."[18] In Finer's terms, Britain has reached the position wished for by John C. Calhoun well over a century ago. Calhoun had dreaded the prospect of the "numerical majority" gaining control of government by means of its majority status and then using its authority to oppress minority sections. To forestall this situation he sought to have government regard interests as well as numbers, allowing each a concurrent voice in the making of laws or a veto in their execution. Finer estimates that the major sectional pressure groups have attained exactly this "concurrent majority" status in British domestic politics.[19] This notion of group veto power provides, in the present study, a means of determining whether and under what conditions the groups' position of power carried over into the formulation of Britain's policies toward European unity in the 1956–7 period.

As will be shown below (Part IV), the evidence from successive periods, 1956–60 (negotiations for a Free Trade Area and then a

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[14] For a detailed treatment, see Political and Economic Planning, *Advisory Committees in British Government* (London: Allen and Unwin, 1960).

[15] In 1965, the Federation of British Industries merged with two smaller organizations, the National Union of Manufacturers and the British Employers Confederation to form the Confederation of British Industries.

[16] These limitations on British pressure group activity are similar to those found by Bauer, Pool and Dexter in their analysis of American groups. Thus in the United States, the role of a trade association leader may be more that of "arbitrator" among forces within his organization than of "statesman"; he also finds himself in a position of mediating between his organization and the outside world and is often caught in a web of conflicting forces (p. 331). In addition, groups are reluctant to take stands on issues unless there is near unanimity within the organization (p. 337). However, British groups do not suffer from the serious lack of money, skills, and information which Bauer, Pool and Dexter find characteristic of their American counterparts (p. 349). The parallels are further limited because the British groups operate within a corporatist political culture so that their legitimacy is enhanced. They also enjoy far greater "density" (percentage of potential membership actually belonging) and "amalgamation" (extent to which the organized have been brought into one body) than do the American pressure groups. See Beer, p. 332.

[17] La Palombara, p. 425. For an alternative conception, based on Dahl's criteria for the measurement of power as the capacity to shift the power of outcomes, see Lieber, Chapter 10.

[18] Finer, p. 27.

[19] Finer, p. 133.

European Free Trade Association), 1961–63 (the Macmillan Government's attempt at Common Market entry), and 1966–67 (the Wilson government's renewal of the Common Market application), indicates a progressive decline in the power of the sectional pressure groups over Britain's European policy making. This decline is caused by effective *politicization* of the European issue, that is, by the perception and treatment of European unity as a matter of major national importance rather than as a relatively specialized or economic question. The result of politicization is to transfer the subject from consideration by the processes of functional representation, in which sectional pressure groups are dominant, to the channel of party government, in which parties play the leading role. Politicization does not require that the issue be subject to partisan dispute, though it may be, for issues can be highly politicized even when there is bi- or tri-partisan accord.

It should be made clear that the term politicization is applied in this paper only when the overall *treatment* of policy merits the label. In one sense, to be sure, all the interactions analyzed here are political in their content, as exemplified by the kind of argument which Almond and Powell offer in their definition of the political system:

> When we speak of the political system we include all of the interactions which affect the use or threat of use of legitimate physical coercion. The political system includes not only governmental institutions such as legislatures, courts and administrative agencies, but *all structures in their political aspects*. Among these are traditional structures such as kinship ties and caste groupings; and anomic phenomena such as assassinations, riots and demonstrations; as well as formal organizations like parties, interest groups, and the media of communication.[20]

As a result, the usage adopted in this study can be regarded as only a specialized case within Almond and Powell's all-encompassing framework.[21] It should also be understood that politicization applies here to the status of those issues which enter into the public domain. Thus, as alternative designations for politicization and nonpoliticization, respectively, we can make use of the terms "publicization" and "privatization," as suggested by Beer.[22]

To be more systematic, it seems useful to identify three necessary indicators of politicization. The *first* of these is the handling of an issue by a primarily political ministry (such as the Foreign Office) rather than by an economic one (such as the Board of Trade). The two types of ministry can be differentiated on the basis of what provision they make for group access. While the Board of Trade accords groups an intimate corporate role, so that extensive consultation occurs between civil servants and their opposite numbers in the sectional groups, the Foreign Office makes no such provision. Although it may receive deputations from groups such as the TUC, these are heard as bodies of substantial importance within the country rather than in their capacity as sectional representatives. Routinized consultation and bargaining are central to the functional representation process; the shift of attention away from the economic ministries tends to prevent their operation.[23]

The *second* indicator is the existence of involvement by the broader public. At a minimum it implies that an issue passes from the exclusive scrutiny of an elite or specialized audience to the notice of the attentive public.[24] This wider attention is reflected in the activity of promotional pressure groups and attention to the issue in the communications media (including the mass press) and in by-elections and opinion polls. The significance of uch involvement is that it conflicts with the ecessarily private process of bargaining between government and sectional pressure roups which makes up functional representaion.

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[20] Almond and Powell, p. 18. Italics in original.

[21] Politicization is also used in different contexts, but with related meanings, by Karl Deutsch *et al., Political Community and the North Atlantic Area* (Princeton: Princeton University Press, 1957), pp. 46–47; and by Ernst B. Haas and Philippe C. Schnitter, "Economics and Differential Patterns of Political Integration: Projections About Unity in Latin America," in *International Political Commuities: An Anthology* (Garden City, N.Y.: Anchor, 1966), pp. 261–262.

According to the P.E.P. study, a nonpolitical subject "is merely one about which politicians do not feel strongly for the time being" (p. 106).

[22] This is also akin to the distinction drawn by E. E. Schattschneider between "socialization" and "privatization" of conflict. Expanding the scope of a previously privatized conflict brings the public, or "audience" into involvement, thus changing the coalition possibilities and decisively affecting the outcome. *The Semi-Sovereign People: A Realist's View of Democracy in America* (New York: Holt, 1960), pp. 2–8.

[23] One listing of 700 central advisory committees presented to the House of Commons in 1949 included only two attached to the Foreign Office and both of these were highly specialized. (The two committees were in the Foreign Office's German Section: The Book Selection Committee and the Scientific Committee). See Allen Potter, pp. 223–225.

[24] "Elite" and "attentive public" are here used in the manner of Gabriel Almond's basic approach. The attentive public is "informed and interested in foreign policy problems, and . . . constitutes the audience for the foreign policy discussions among the elites." The "policy and opinion elites" are "the articulate policy-bearing stratum of the population which gives structure to the public, and which provides the effective means of access to the various groupings." *The American People and Foreign Policy,* rev. ed. (New York: Praeger, 1960), p. 138.

The *third,* and most important of the indicaors, is the participation of the political parties. ince public opinion sets few limits on foreign olicy[25] the crucial element in politicization, nd in the limitation of group influence, beomes the political parties. Party involvement nterferes with the exclusively technical considration of an issue in a closed relationship beween administrative department and sectional ressure group. It also provides the opportunity —though certainly not the assurance—that udgments may be rendered more often on the asis of an interpretation of broad national inerest than on some kind of cost-benefit calculaions important mainly to sectional interests. This impact of party involvement stems not nly from the fact that parties are organized round some distinctive conception of the ommon good—no matter how vague or vestiial—but also because those interests which the arties do heed in their search for support and n bidding for votes have to be aggregated with umerous other interests. Inherently this aggreation implies dilution. For politicization to beome *effective* in the limitation of group influnce, one other dimension must be present: the ssue involved must be treated as falling within he realm of diffuse or general-purpose politics ather than of functional or special-purpose olitics. The distinction resembles that made by tanley Hoffman between "high politics" and "low politics."[26]

If the above factors are indicators of politiciation, what are its causes? The onset of politiization appears to be determined by a combiation of external events and conscious choices. Thus a pressing international situation may hrust a subject into the forefront of national ttention, or at least make such treatment posible. What events do is to create a propensity oward politicization. They constitute the necessary, if not always the sufficient, condition. Internally, a conscious choice by political or governmental figures is almost always essential before politicization can occur. For example, despite the importance of the EEC, neither Conservative nor Labour leaders consciously chose to treat European unity as a salient political matter until 1961.

The importance of the above treatment of the circumstances in which pressure groups rather than parties can be expected to influence policy is that pressure groups are different from parties in ways that directly affect issues such as those involving European unity. Basically, pressure groups influence policy formation whenever the criteria for judgment are based more upon the foreseeable balance of economic gain and loss than upon broader conceptions of national interest or political benefit. As a result, policy initiatives which threaten identifiable costs to significant interests, even though these may represent a minority of the sector involved, are likely to be opposed. Comprehensive groups, such as the CBI and NFU, will tailor their positions so as not to outrage important sectors of their membership; and in a setting which remains nonpoliticized, political parties will play little role in the policy process. The result, therefore, is likely to be that profound new ventures such as those involving European unity cannot be made so long as pressure groups provide the main constituency for policy makers.

While the group politics theorists do not deal with the phenomena of supranational integration per se, it does seem possible to extend their analysis in order to do so. Based on the above arguments, such an extension would logically imply that when an integration issue is nonpoliticized (i.e., treated as a technical matter), groups will tend to influence policy formation, because cost-benefit calculations become the criteria for judgment, and movement toward integration is likely to be hindered. Put more economically, this extension of the group politics position gives rise to an hypothesis: *If interest groups influence policy formation, then progress toward integration is likely to be impeded.*

By implication, progress toward any major new initiative in European unity (that is, obtaining a British decision to join an integrated European organization) will require that an issue become effectively politicized.

III. The Functionalist Hypothesis

Functionalism shares basic assumptions with group politics in the sense that it implies there will be group influence if an issue (specifically one involving integration) is nonpoliticized. But, unlike the group politics analysis, it finds nonpoliticization to be advantageous for integration.

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[25] British voters tend to be bi-partisan on foreign olicy, and concentrate their attention on domestic natters. Their tendency to vote on the basis of donestic issues is analyzed in Max Beloff, *New Dimenions in Foreign Policy: A Study in British Administraive Experience, 1947–1959* (London: Allen and Unvin, 1961), p. 15; Jean Blondel, *Voters, Parties and Leaders* (Harmondsworth Middlesex: Penguin, 1966), pp. 75–79. 81–83, 87; and Kenneth Younger, "Public Opinion and British Foreign Policy," *International Afairs,* 40 (January, 1964), 22–23.

[26] See, e.g., "European Process at Atlantic Crossourposes," *Journal of Common Market Studies,* 3 (February, 1965), 92.

The basic arguments of functionalism are most commonly associated with David Mitrany and Ernst Haas.[27] The essence of Mitrany's position is that the surest route to world peace is to organize a multitude of international organizations around functional tasks such as health, transport, and postal services.[28] As governments eventually cede more and more of their tasks to these worldwide organizations, a diminution of national antagonisms will result. Functional institutions will gradually bring about a sense of community; ultimately, peace, world government, and political integration will follow. Mitrany's thought is usually contrasted with that of the federalists, but he also stands apart from present functionalist theory in certain respects, particularly in his stress upon functional organizations which are universal rather than regional in their membership. Nonetheless, that aspect of the Mitrany thesis which is most relevant to the present discussion and which is shared by contemporary functionalists, is that the surest route to political integration is to depoliticize the subject by concentrating on technical or functional tasks.

For Ernst Haas, who has developed important modifications of Mitrany's approach, the essense of functionalism is that step-by-step economic decisions are superior to crucial political choices and that the operation of ever more controversial policies, starting from a shared interest in economic welfare, will, with a certain automaticity, bring about the establishment of a new central, supranational authority regardless of the wishes of individual actors. Haas's functionalism assumes that economic self-interest is more important than political commitment and that unintended consequences and incremental decision making are more effective in bringing about integration than are purposive behavior or the constructic of grand designs. Basing his analysis upon tl Western European experience, Haas identifies natural economic progression which has led tl six members of the European Economic Con munity (EEC) from their initial common con munity treatment of coal and steel (in the Eı ropean Coal and Steel Community) to refrige: ator tariffs, and then to chickens, to monetaı policy, and ultimately to a mutual interest i the operation of the business cycle.

Initially, Haas argued that in modern demc cratic industrial society, particularly that c Western Europe, there was no longer a di tinctly political function, separate from econon ics or welfare or education, that existed in th realm of foreign policy, defense, and constitu tion making. He also viewed the advent of su pranationality within the EEC as symbolizin the "victory of economics over politics," thu signaling the demise of the ethnocentric natior alism which preferred guns to butter, passion t reason, and excited demands to statistical baı gaining.[29] During the past decade, Haas ha modified his dismissal of the political functior He acknowledges that *The Uniting of Europ* erred in assuming the permanent superiority c step-by-step economic decisions over crucia political choices, and takes the position tha while integrative decisions based on high poli tics are more durable, in the absence of statesman with the vision to weld disparat publics together "we have no alternative if w wish to integrate a region, but to resort to grad ualism, to indirection, to functionalism."[30] I sum, he maintains that subject to amendments the expansive logic of functionalism remain valid.[31]

Yet Haas does characterize the relationshi between economic and political union as "continuum."[32] And his functionalism contin ues to imply that a kind of benign invisibl hand will operate to bring about supranationa integration in a nonideological, depoliticize economic setting. Since government concen trates increasingly on technical questions of aı economic nature, political life more and mor revolves around interest groups seeking narrov vantages, and public policies are regarded as e outcome of these maneuverings along with her economic and political forces.[33] Haas resses the growth of interest groups across ontiers, and a chief conclusion of his main ork is that group pressure will shift from a tional to a community level and have the fect of stimulating integration as groups orga- ze across national boundaries to influence licy.[34] His pluralistic thesis holds that in ad- nced industrial democracies, a larger political mmunity can be developed if crucial expecta- ons and behavior patterns of key groups can refocused on a new set of central symbols d institutions; and over time, political loyal- es, following economic interests, will grad- ally become attached to new supranational in- itutions.

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[27] See especially, Mitrany's *A Working Peace System: An Argument for Functional Development of International Organization* (London: Oxford University Press, For the Royal Institute of International Affairs, 1943); also Haas's *The Uniting of Europe: Political, Social and Economic Forces, 1950–1957* (Stanford: Stanford University Press, 1958), and his *Beyond the Nation-State: Functionalism and International Organization* (Stanford: Stanford University Press, 1964).

[28] For a useful summary of Mitray's functionalism and its differences from later interpretations, see Andrew Wilson Green, "Review Article: Mitrany Reread with the Help of Haas and Sewell," *Journal of Common Market Studies,* 8 (September, 1969), 50–69.

[29] "Technocracy, Pluralism and the New Europe," i *A New Europe,* ed. Stephen Graubard (Boston Houghton Mifflin, 1964), p. 71.

[30] "*The Uniting of Europe* and the Uniting of Latii America," *Journal of Common Market Studies,* : (June, 1967), 327–328.

[31] Haas, "*The Uniting of Europe . . .*," p. 321

[32] Haas and Schmitter, "Economics and Differentia Patterns of Political Integration . . ." p. 261. For Haas' revisions to his original theorizing, see "*The Uniting o Europe . . .*" and his 1968 Preface to the reissuec edition of *The Uniting of Europe.*

It is true that functionalism has been more oncerned about integration within existing ommunities, where processes such as spillover o in fact seem to take place, than about the eographic expansion of integration or the con- ruction of new communities.[35] Leon Lindberg nd Stuart Scheingold have characterized the ormer type of integration, to which Haas's ex- ansive logic of functionalism has been applied, s "forward-linkage growth." This process in- olves primarily incremental change and the rojection of well-established trends. They label he latter type or process as "systems transfor- ation." This category incudes not only the ad- erence of new members to an established ommunity, but also other large changes which ean the introduction of new constituencies uch as those involving a notable increase of ntegrated activities within an existing commu- ity.[36] In fact, the functionalists generally have ot sought to extend their argument to deal ith the adherence of a new member such as ritain to an established supranational entity. ut in seeking to analyze the British case, we onetheless may find it worth the effort to ex- end the functionalist approach in order to de- ermine whether it could also account for this ind of effort at systems transformation. Such n extension would logically imply that whenever an integration issue is nonpoliticized, groups will tend to influence policy formation, because cost-benefit calculations become the criteria for judgment, and movement toward integration is likely to be facilitated. Put more economically, this extension of the functionalist position gives rise to the following hypothesis: *If interest groups influence policy formation then progress toward integration is likely to be facilitated.* The difference between the hypotheses here imputed to the group politics and functionalist approaches is that while both agree upon a significant role for interest groups in the absence of politicization, the first hypothesis asserts that this role will hinder integration, while the second predicts the opposite—that it will facilitate integration. The difference of orientation is evident in the notion of group veto power identified by the group politics analysis, as compared to the stress on technocratic politics and the attribution of an integrative group role by the functionalists.

With these two hypotheses in mind, it is now appropriate to see how each one holds up against the British experience. The period concerned begins with Britain's effort to come to terms with the newly established European Economic Community by means of the ultimately abortive Free Trade Area during the years from 1956 to 1958. It then continues with the establishment of the European Free Trade Association during 1959–60, the effort of the Macmillan Government to join the Common Market beginning in 1961 and vetoed by President de Gaulle in January 1963, and—following a hiatus of more than three years—the similarly fruitless renewal of Britain's application by the Wilson Government in 1966–67.

IV. Pressure Groups and British Policy: 1956–67

A. The Free Trade Area and the European Free Trade Association. During the years from 1956 to 1960, the period of policymaking for the Free Trade Area and the European Free Trade Association, the European issue remained almost completely nonpoliticized. Neither external events nor the choices of party or governmental leaders compelled a political treatment. The three indicators reflected this lack of politicization. First, at the administrative level, policy formulation and international negotiations were conducted by the Board of Trade with some participation by the Treasury. The Foreign Office did not take part. Second, generalized debate and involvement by the broader public were largely absent. The mass press gave little coverage to the FTA and EFTA, particularly in comparison with its later handling of the Common Market question, and there was no mobilization by promotional pressure groups. Third, the parties remained largely uninvolved, particularly because they shared similar official attitudes and because the subject of European unity was widely perceived as economic in content.[37]

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[33] See Michael J. Brenner, *Technocratic Politics and he Functionalist Theory of European Integration* Ithaca, New York: Cornell University Center for nternational Studies, 1969), p. 5.

[34] According to Haas, perhaps his "chief finding is hat group pressure will spill over into the federal phere and thereby add to the integrative impulse." *he Uniting of Europe,* 1968 edition, p. xxxiii.

[35] Brenner discusses this on p. 8.

[36] *Europe's Would-Be Polity: Patterns of Change in the European Community* (Englewood Cliffs, N.J.: Prentice-Hall, 1970), p. 244.

Interest groups enjoyed concurrent majority power during this period; they succeeded in maintaining a political position similar to the one they customarily occupied in domestic politics. That is, they exerted influence because the executive found it necessary or desirable to bargain for their cooperation. Though the groups paid more attention to technical aspects than to overall policy, this was consistent with their usual pattern of operation on domestic matters.

Throughout this period, all three of the major groups were hostile to the idea of outright Common Market membership. For example, the TUC was completely opposed, fearing that the Common Market would constitute a first step toward complete integration and the development of a central authority which could overrule national governments on important economic matters. TUC leaders felt such a change would be undesirable since they viewed most governments of the Six as having less favorable ideas on economic and social policy than the British government, albeit a Conservative one. Even more important for the TUC—as for the other sectional interest groups—was the structural change involved. A transfer of authoritative decision-making power to a supranational body would drastically weaken the effective influence of the TUC. In a new European Community, the sectional veto which British trade unions often hoped to exercise upon their own government's policies would no longer be so significant. That is, even though operation of a concurrent majority might persist at the national level, its existence would be far less important because the determination of economic policies would have been removed to a supranational authority upon which the British government—and the British public—would have only partial influence.

During the years from 1956 to 1958 Britain unsuccessfully sought to negotiate establishment of a wide Free Trade Area linking the six members of the newly formed Common Market with the other Western European members of the OEEC. What is noteworthy about th period is the extent to which the pressu groups not only resisted any thought of membership in a supranational EEC, but also took very cautious position toward even the ve limited European arrangements in the propose Free Trade Area. Thus, in the case of the Fe eration of British Industries, while the larg business firms and the FBI leadership were f vorable to European free trade, many of th smaller firms expressed concern about meetin increased competition; given the nature of th FBI as an organization, this apprehensio meant that its policy had to remain a cautio one. As for the National Farmers' Union, categorically refused to consider any Europea agreement which would include agriculture. B its success in delaying until October 1957 governmental move to meet demands by th Common Market countries for some provisio for increased agricultural trade, it played a significant role in hindering European agreemen on the FTA. Even the TUC's policy toward th FTA concentrated almost entirely on safeguards for full employment instead of on th opportunities for economic growth offered b the proposed trading arrangements.

There is every reason to judge, as Miriar Camps does, that the government felt its mai task lay in defeating internal opposition to fre trade and in winning over business and labo rather than concentrating on gaining agreemen with the Six.[38] The government's decision to exclude agriculture from the initial FTA proposals prompted a very explicit acknowledgmen of the groups' importance from a Labour MP

> The only really effective argument which I hav heard for excluding foodstuffs from the scheme i that if we are going to have trouble with pressur groups and with the FBI, why should we hav trouble at the same time with the NFU? From tactical point of view, there is much to be sai for taking on these pressure groups one at a time.[39]

Most of the European countries wanted Britain in Europe for political reasons, but France was unwilling to pay Britain's price and later vetoed the FTA in November 1958. The question then, is why the UK required such a price which was ultimately to preclude the successful establishment of a Europe-wide FTA. The answer is to be found in the requirements of British pressure-group politics. The groups were highly active participants in the formulation of British policy, by virtue of their concurrent majority powers, yet they played a role that diectly opposed European integration and emphasized sizeable economic safeguards, even pon the quite limited FTA arrangements.

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[37] E.g., Edward Heath told the 1960 Conservative Conference that the government had sought to create the FTA and EFTA "for economic reasons." National Union of Conservative and Unionist Associations, *79th Annual Conservative Conference* (October 12–15, 1960), p. 61.

[38] *Britain and the European Community*, pp. 104–105.

[39] Fred Mulley in Hansard, *Parliamentary Debates* (Commons), 561 (November 26, 1956), c. 80.

In 1959–60 (the period during which the European Free Trade Association, or EFTA, was formed), the sectional pressure groups gain played an intimate role in the shaping of policy, one which went well beyond a mere veto power. In fact the Federation of British ndustries, through meetings with its Scandinavian counterparts during the winter of 958–59, played a substantial part in moving he British government toward the establishment of the EFTA.[40] The FBI, in close cooperation with the NFU overtly worked to secure he establishment of EFTA at a time when government probings were still without commitment. FBI activities gave prominence to the EFTA option, and at the very least this advocacy made it possible for the government to proceed with EFTA in the knowledge that such a course of action would benefit from firm FBI support. In the face of industrial disapproval, he EFTA choice would have been unlikely.

As in the case of the Free Trade Area, the EFTA issue remained almost entirely nonpoliticized. Because of the economic and commercial treatment of the European issue throughout 1959, the FBI's involvement and importance came naturally. To describe the government as having taken the more narrowly economic view is no idle generalization. In their failure to adopt a wider outlook, British leaders paid little attention to the political dynamics behind the Common Market; they seldom displayed awareness of the Common Market countries' sense of building Europe and of laying to rest the antagonisms which had caused two World Wars.

While the interest groups played a significant role in a depoliticized setting, their participation was not such as to contribute to integration, particularly in terms of Ernst Haas's definition, widely employed by the functionalists, which sees integration as:

> the process whereby political actors in several distinct national settings are persuaded to shift their loyalties, expectations, and political activities toward a new and larger center, whose institutions possess or demand jurisdiction over the preexisting national states.[41]

But the merely intergovernmental arrangements of the EFTA did not begin to provide a setting in which institutions or arrangements existed toward which previously national loyalties might be transferred. By continuing to oppose British participation in genuine integration of the EEC variety, interest-group activity impeded integration. Indeed, the groups contributed to the formulation of a policy and to the establishment of an organization which actually hampered British efforts to come to terms with the Six. The existence of EFTA complicated Britain's task in seeking Common Market membership, both technically and—as it contributed to the Six's suspicion of British motives—psychologically.

It thus becomes clear in looking at the 1956–60 period that a nonpoliticized treatment of European policy would not suffice to move Britain into an integrated European arrangement. As long as the setting remained nonpoliticized, interest groups would play a significant role in policy making, at least to the extent of preserving veto power, but this role would be in opposition to rather than in the direction of genuine integration. The contrast with 1960–61 is dramatic, for no British movement would be possible until the European issue became politicized.

B. Common Market I: 1961–63. In the case of Britain's first Common Market application, during the period 1961–63, the key decision was taken by Prime Minister Macmillan when he decided to abandon Britain's aloofness from European integration and to seek membership in the European Economic Community. The overwhelming preponderance of views by elite respondents interviewed for the present study was that political motivations (both short- and long-run) predominated. To a slightly lesser extent, these respondents also concurred in the observation that once the decision was made, Macmillan presented the European choice to the British public as though Common Market membership were primarily an economic matter. The result was that there were really two distinctly different phases to the British approach, the first being that of the initial decision to seek entry, the second consisting of the period of actual negotiation. Treatment of the European issue, the role of pressure groups, and the nature of the policy process differed sharply between these two phases.

As for the initial decision, its aims corresponded with the conclusions of an early 1960 report by the Economic Steering Committee under the chairmanship of Sir Frank Lee

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[40] The original seven members of EFTA were Sweden, Norway, Denmark, Switzerland, Austria, Portugal and Britain. The arrangements resembled those which Britain had sought for the abortive FTA, namely freer intragroup trade, a minimum of institutions and of integration, and no harmonization of external tariffs.

[41] "International Integration: The European and the Universal Process," in *International Political Communities*, p. 94.

(then newly appointed as Joint Permanent Secretary to the Treasury). This interdepartmental group of senior civil servants had been assembled to study possible association with the Six, but concluded that Britain should seek outright membership in the Common Market, for primarily political reasons.[42] The Committee regarded the economic arguments as at best balanced; for them, entry was a political, not an economic question. They expected Britain to exert leadership within a united Europe, thus regaining a certain measure of world influence and even improving her transatlantic ties.

In short-range political terms, European integration offered a novel issue on which to set the Conservatives apart from the Labour Party, following a period in which the two major parties had drifted toward the political center as factors of ideology and class diminished. Finally, Macmillan himself, seeking a lasting contribution with which to cap his political career, had long been favorable toward European unity.[43] Macmillan had become Prime Minister in early 1957, and having disposed of the urgent tasks of liquidating the Suez venture, assuming control of the Conservatives, and rallying his dispirited Party for an overwhelming victory in the 1959 election, he felt that circumstances in 1960–61 at last seemed appropriate for seeking Common Market entry.

The available evidence indicates that the Prime Minister made his decision in late December 1960. It is quite clear—especially in comparing the chronology of public statements and private documents of the sectional interest groups—that Macmillan's decision antedated group advocacy of Common Market membership. This finding offers no comfort for the functionalists. The distinctly political function is evident; its existence is a prime requisite for the critical transformation of British policy from opposition to European integration to participation in it. The key point here is that the July, 1961 announcement that Britain would seek EEC entry reflected a political judgment, made by political authorities before pressure groups had strongly articulated their own interests. In particular it meant that considerations of broad national interest, construed in terms of Britain's overall international position, took precedence over cost-benefit calculations about the effects of Common Market membership on individual economic interests. It also reflected the existence of policy powers held by the Prime Minister and Cabinet which could be exercised relatively free of the constraints tha interest groups typically imposed on lesser domestic issues. But because Macmillan would not or could not make the case to the British public in grandly political or Churchillian terms (as opposed to less ambitious commercia ones), the subsequent formulation of Britain's negotiating position offered the opportunity for a powerful pressure-group role.

Politicization of the issue of European unity was evident throughout the period of the firs Common Market application, and the three indicators reflect this fact: first, the negotiations were no longer handled by ministers with primarily economic departmental responsibilities but by Edward Heath, then number two man at the Foreign Office. Second, the broader public was drawn into the debate. Promotional pressure-group activity became widespread; mass circulation papers such as the *Daily Mirror* and *Daily Express* launched strident campaigns for and against entry; and polls and by-elections reflected the Common Market's position as a highly visible subject of debate. Third, the political parties became deeply involved and even made European unity a matter of partisan contention. Yet while there was full politicization of the European issue, in the sense of publicization, during the actual negotiation period, politicization did not become *effective* because Macmillan chose to treat the Common Market as an economic issue. He operated, in the words of Richard Neustadt, "by disguising his strategic choice as a commercial deal."[44] The Prime Minister's decision to deal with the issue as one of functional politics rather than of general purpose politics, facilitated the persistence of functional representation and hence assured a significant pressure-group role in shaping the practical terms of entry to be sought by Britain.[45]

The increasing opposition to Common Market entry by the Labour Party also enhanced the significance of sectional demands because, lacking bipartisan reinforcement for a policy which he had described in economic terms, Macmillan needed to assure himself of the fullest possible support within his governing Conservative Party. In this case, because the Tory constituency was composed very considerably of the agricultural, industrial, and commercial portions of the society, sectional groups such as the NFU and FBI actually were able to exert influence via the Prime Minister's own party.

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[42] Camps, *Britain and the European Community*, pp. 280–281.

[43] Macmillan later wrote in his memoirs, "About Europe, regrets still haunt me," and he recalled that he had written Churchill in protest when the newly elected Conservative government of 1951 failed to move Britain toward the European Coal and Steel Community. *Sunday Times* (London), July 31, 1966.

[44] "Whitehouse and Whitehall," Paper delivered at the 1965 Annual Meeting of the American Political Science Association, Washington, D.C., September 8–11, p. 9.

[45] The negotiations also became mired in detail, a prime area of group operation, because the Government sought to negotiate *à sept* (rather than regard the Six as a single group with a coherent position), because it felt obliged to negotiate on behalf of the Commonwealth, and because the openness of the negotiations frequently involved Edward Heath in an embarrassing dialogue with the groups.

The relationship between domestic and international forces is especially visible in the 1961–63 period, with Macmillan becoming increasingly trapped between domestic political needs and the necessities of the Brussels negotiations. An easier domestic situation would have allowed the Government to accept the EEC's Rome Treaty more unreservedly. This acceptance in turn would have facilitated mutual concessions by the Six and a speedier course for the negotiations. As Kenneth Waltz has observed, however, Macmillan failed publicly to confront the European issue as one which challenged long-existing national notions.[46] Instead, in order to gain domestic approval for Common Market entry, he pledged to obtain safeguards which were virtually incompatible with full British membership in the EEC. The British reservations were not dictated by public opinion; they were established on the basis of consultations with the major interests.[47] The consequences of this approach were to hinder the negotiations at Brussels, and then to undercut the Government's domestic position because the necessary retreat from the initial conditions was interpreted at home as a series of surrenders. These difficulties facilitated the subsequent de Gaulle veto.

Once again, interest groups exerted an influence which cannot be said to have promoted integration. The case of the FBI is the most interesting. During 1956–60, British business had perceived the trend toward European unity more as a threat than an opportunity, though near the end of 1959 a number of larger business firms became more favorable toward Europe because of their concern with stagnating British Commonwealth trade as contrasted to growing opportunities within the EEC. Smaller firms remained more hostile, whether because of vulnerability in the home market or less imaginative leadership, but they sometimes expressed their fears in political rather than economic terms. By the spring of 1961, the heads of giant firms such as Imperial Chemical Industries, General Electric Corporation, and British Motor Corporation had begun to advocate outright entry into the Common Market rather than some more limited form of association. The FBI, caught between conflicting views, issued a major report in July 1961, two weeks before the Government's announcement. This report stressed the need for Commonwealth and EFTA safeguards, termed many of the EEC's agricultural arrangements unacceptable, expressed a preference for intergovernmental cooperation rather than common European institutions, and also took note of numerous "negotiable" lesser problems.[48] While the FBI had taken one important step in its willingness in principle to accept a common external tariff, even this declaration was negatively phrased.[49] The whole tenor of the FBI report was legalistic and parochial; the terms it stipulated remained quite irreconcilable with Common Market membership—especially because of the position on agriculture and common institutions. At the same time however, a survey of 130 large firms found more pro-EEC sentiment.[50] The leaders of some of Britain's largest industries were clearly moving toward European unity faster than the FBI was, though even these industrialists moved no more rapidly than Prime Minister Macmillan.

During the actual period of negotiation for Common Market entry, the role of the FBI vis-à-vis the government and its own membership shifted somewhat. The FBI maintained a close relationship of sustained consultation with the government, expressing concern over vulnerable sections of industry and also offering technical advice. Because outright hostility on the part of business would have prevented the Common Market application altogether, the British government needed to devote considerable care and attention to entertaining the representations and occasional protests of the FBI (and of the TUC as well) in order to insure that they continued to provide support, or at least acquiescence, for entry. With the passage of time, and following the lead of the larger industries, the FBI became increasingly pro-EEC and directed substantial effort at educating and persuading its own membership as to the benefits of entry.

It was the National Farmers' Union, having the most to lose, which exerted the greatest influence upon the British position. The farmers may not have been able to veto entry itself, but they did succeed in shaping the conditions under which Britain sought that entry. The NFU first obtained a governmental pledge, in a January, 1961 White Paper, that the government would be concerned to see whether closer European unity could be achieved "without sacrificing the vital interests of UK farmers and horticulturalists."[51] Next, in the official July, 1961 announcement, the Prime Minister incorporated safeguards for British agriculture as one of three essential British conditions in negotiations with the Six.[52] Finally, and perhaps more importantly, agriculture received its most specific pledge in the speech made by Edward Heath in submitting Britain's application to the Ministers of the Six at Paris on October 10, 1961. In a speech otherwise regarded as one of wholehearted commitment to Europe, Heath enumerated three necessary safeguards for the vital interests of British farmers: a transition period of 12–15 years, British retention of the ability "to use such measures as are necessary to safeguard our farmers' standard of living," and special arrangements for horticulture.[53] Eventually, the problem would be that the Six would find each of these elements largely unacceptable, and Britain would have to retreat from them if she wished to reach agreement.

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[46] *Foreign Policy and Democratic Politics: The American and British Experience* (Boston: Little Brown, 1967), p. 266.

[47] This point is emphasized by Lord Windlesham in *Communication and Political Power* (London: Jonathan Cape, 1966), p. 158.

[48] Federation of British Industries, *British Industry and Europe* (London, July 1961).

[49] "In principle we do not oppose the suggestion for a common or harmonized tariff put forward by HMG." Federation of British Industries, p. 3.

[50] *Sunday Times* (London), July 16, 1961.

Thus from July, 1961 to the January, 1963 de Gaulle veto, the British government was seeking to placate specific economic objections of the major interest groups in order to insure that Common Market entry would have domestic political support. The groups could not assert their inherent involvement in the central political issue of whether Britain should merge her destiny with that of the Europeans; but because the issue was incompletely politicized (i.e., treated as functional or economic, despite publicization), these groups still could claim direct concern with portions of the European issue involving trade, full employment, and agricultural arrangements. The groups thus retained an approximate concurrent majority role in the formulation of Britain's negotiating position.

C. Common Market II: 1966–67. The orientation of the Wilson government toward Europe provides some useful contrasts to the 1956–60 and 1961–63 periods. During the 1966–67 attempt at Common Market entry,[54] the effectiveness of politicization rendered group interests relatively insignificant. Unlike Macmillan, Harold Wilson was thoroughly political in the way he handled the European negotiations and the way he presented them to the British public, as well as in his decision criteria.[55] Wilson's effort was thus virtually unimpeded by the requirements of pressure-group politics.

This difference of approach resulted from a combination of external events and conscious choices. The urgency of events, particularly the collapse of alternatives, resulted in a strong propensity toward politicization; and the nature of Wilson's choices, most notably the decision to treat the European commitment as a major national departure, (i.e., as a matter of general, or high, politics rather than of functional, or low, politics) resulted in an effective politicization.

Sectional pressure groups therefore found themselves in a position of unprecedented weakness. Effective politicization had closed the channel of functional representation, and a number of conditions, including the existence of bipartisan agreement over European policy, discouraged group access through the channel of party government. Unlike Macmillan, Wilson made no pledges to pressure groups; he also sought entry with far fewer conditions.[56]

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[51] Cmnd. 1249 (Her Majesty's Stationery Office, December 19, 1960), paragraph 37, cited in *British Farmer*, No. 163 (January 7, 1961).

[52] Hansard, *Parliamentary Debates* (Commons), 645 (July 31, 1961), c. 1483–88.

[53] *The United Kingdom and the European Economic Community*, Cmnd. 1565, Vol. 36 (HMSO, November, 1961), pp. 14–15.

[54] The Labour government's approach to the EEC began in November 1966 with exploratory talks. The application was formalized in May 1967, then vetoed by de Gaulle in November 1967. Prime Minister Wilson initiated a renewal of the application in 1969–70, and Labour was then defeated in the June 1970 General Election. The incoming Heath government continued with the negotiations and reached agreement with the Six in June 1971. The later phase is beyond the scope of this paper.

[55] This political emphasis was quite obvious. See, e.g., Kitzinger, *The Second Try*, pp. 9–10.

[56] The unconditional nature of the Labour government's application is particularly visible when its terms, as set out by George Brown at The Hague on 3–4 July, 1967, are contrasted with those outlined by Edward Heath in Paris on 10 October, 1961. Thus, for example, on agriculture, Brown accepted the EEC's Common Agricultural Policy without safeguards, while Heath sought to retain Britain's existing safeguards for her farmers. On the Commonwealth, Brown cited only New Zealand and the need to avoid defaulting on the Commonwealth Sugar Agreement; Heath listed each country, requested "comparable outlets," and said Britain could not join if trade was cut with a severe loss to the Commonwealth. On EFTA, Brown asked only one year's standstill for its members "to make their own arrangements," while Heath said Britain could not join unless all seven members "could participate, from the same date, in an integrated European market." On collective action, Brown ac-

Moreover, Wilson centered the work of his neotiating team in the Cabinet Office, where he imself would be in control. The sectional inerests found themselves fobbed off upon a conultative committee operating through the short-lived) Department of Economic Affairs nd providing them with occasional briefings nd only *pro forma* consultation rather than enuine bargaining. Groups might still influnce lesser technicalities, but they were left without a vestige of concurrent majority powrs.

One lesson suggested by the 1966–67 period s that the perceived precedence of external inernational factors dictated that the European pproach would be unimpeded by the stipulaion of conditions. In its Common Market approach, the Macmillan government had acnowledged no such predominance of external oncerns and had consequently devoted more ttention to domestic "consensus" than to international "compatibility."[57] But Prime Minister Wilson's desire to avoid presenting President e Gaulle with a genuine pretext for another veto equired a de-emphasis of domestic considertions, which, in turn, meant the demise of unctional representation. Labeling the changed reatment as effective politicization implies that broadly construed national interest had beome the dominant concern; this heightened national interest transcended the previous patern in which sectional pressure groups had exrcised effective bargaining power in the abence of a clear-cut invocation of Britain's naional interest. As in the case of the 1961 Common Market application, political authorities ad again made the crucial decision in advance f, or without real regard for, the sectional ressure groups' strong articulation of their poitions. But unlike the earlier period, during 966–67 effective politicization continued beond the initial decision phase and thus shaped he context in which the actual terms of the aplication were formulated.

V. Conclusions

The evidence of these successive periods is hat group influence in foreign policy is inversely proportional to effective politicization. The sectional pressure groups' domestic corporatist role, as expressed by the concurrent majority notion, appears to carry over into the realm of foreign policy making unless the issue becomes effectively politicized. The relationship between group influence and politicization, however, may be somewhat more complex than either the group politics or functionalist hypothesis implies. The evidence of the period following Macmillan's July, 1961 announcement that Britain would seek Common Market entry is that if political authorities do not consciously choose to treat an issue as broadly political, so that considerations of general national interest take precedence, then the sectional interests are likely to reassert their involvement in a situation where cost-benefit calculations become important criteria for judgment. The question of bipartisanship also assumes some importance in the limitation of group influence. As we have seen, Macmillan's hand was weakened by the absence of Labour Party backing in 1961–63. This lack forced him to seek additional support by making a succession of pledges, and it facilitated a measure of pressure-group access via the party government process (as opposed to the usual concentration of these groups on the administrative departments of government by means of functional representation). In contrast, Prime Minister Wilson's position was strengthened by opposition support for Common Market entry in 1966–67. Nonetheless, it would be wrong to conclude that simple bipartisanship, rather than the more complex phenomenon of politicization, best accounts for restricted pressure-group access. The simpler explanation proves inadequate when applied to the entire 1956–60 period, during which bipartisanship in a completely nonpoliticized setting coincided with an exceedingly high group access.

How then do the suggested group politics and functionalist hypotheses compare in terms of their prediction of the role of interest groups in the integrative process? The results indicate a confirmation of the group politics hypothesis —at least within the confines of the British case. During the FTA and EFTA phases, and again during the first set of negotiations for Common Market entry, pressure groups did influence policy, and they functioned to restrain Britain's movement toward participation in integrated European ventures. They exercised this role first by means of direct veto powers, then indirectly by operating to influence and qualify the terms of British association or membership.

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epted the Rome Treaty and all "regulations, directives nd . . . decisions taken under it"; Heath accepted he Treaty but proposed to negotiate over agreements eached among the Six since then. See Nora Beloff, "What Happened in Britain After the General Said No," in Uri, *From Commonwealth to Common Market*, pp. 81–83.

[57] The terms are those of Wolfram Hanrieder, "Compatibility and Consensus: A Proposal for the Conceptual Linkage of External and Internal Dimensions of Foreign Policy," *American Political Science Review*, 61 (December, 1967), 971–982.

Why were these groups generally opposed or at best guardedly favorable toward integration throughout a period when British membership of the EEC offered economic prospects ranging from breaking even[58] to clear and substantial advantages? Evidently, these group orientations reflected organizational rather than strictly economic considerations. While the CBI, NFU, and TUC are powerful and important bodies, as the peak organizations in their fields, they are amorphous and unwieldly. Group leaders lack the centralized authority which exists within an individual corporation, and policies tend to be developed on the basis of a lowest common denominator.[59] The groups' political muscle is consistently employed to protect sectors which could be sacrificed as uneconomic, even though the resultant balance of gain and loss might leave businessmen or farmers or trade unionists, on the whole, much better off.[60] An appropriate example is the operation of the National Farmers' Union during the 1961–63 negotiations for Common Market entry: British agriculture in general stood to gain from Common Market membership, but the NFU's considerable powers were employed to secure governmental pledges of safeguards for the horticultural sector, which by virtue of Britain's climate and geography was simply not in a position to compete successfully within an expanded EEC.

Another set of group considerations which is not exclusively economic involves the status of interest groups within the British political process. The groups explicitly claim a veto power within a setting of collectivist politics, and they are prone to resist any shift which, while promising material benefits, might threaten this status. Integration theorists have celebrated the phenomenon of Community-wide pressure groups emerging to press their common interest upon the EEC Commission at Brussels, as the Community organs have taken on decision-making functions once exclusively the province of national governments.[61] This process is regarded as offering a new channel for sectional groups to influence policy outcomes, but even in some future federal Europe with effectiv transnational interest groupings, a single n tional organization still would have consider bly less influence than in a domestic context. is thus readily understandable that durin 1956–60 a group such as the TUC preferre that European cooperation proceed on the pa tern of the intergovernmental OEEC rathe than on the basis of supranational integratio During 1961–63, the NFU expressed a simil concern in its orientation toward the Commo Market.

Finally, in assessing the reluctance of Britis interest groups to support European integr tion, one must consider the background var able of political culture. These groups hav been characterized by a resistance to innov tion, a distrust of complex written constit tional schemes, an insularity, and a sense of n tional self-reliance that in Britain—unlike th Continental countries—had not been discre ited by occupation or defeat during World Wa II. In all these respects, the groups shared a orientation inherent in British life as a whole.

Taken together, their organizational natur their position in corporatist politics, and pe haps the background effects of political cultur make interest groups highly complex and au tonomous in their operations. To the exten that the functionalist view defines intere groups as representing more or less a mechan cal vector sum of economic forces, it clearl needs modification.

As for the suggested hypothesis based on a extension of functionalism, the results of th British case require a rejection. Rather than facilitating integration, the weight of section pressure group influence, was almost alway opposed or equivocal. Functionalist theor holds that interest groups will favor integration particularly if economic benefits are involved but in the British experience, so long as th country did not belong to a supranationa grouping, this generally was not the case.

More broadly, functionalist theory, from it origins in Mitrany's writing, has viewed depoliticization along with a functional or technica treatment of issues, as the most likely route t integration. But to the extent the British operated this way in their relationship to Europ (in 1956–60, and again following the Macmillan decision in 1961–62), the approach simply did not work. Political judgments were indisputably the sine qua non for any significan British movement toward participation in European supranational integration. The fact tha political authority, moved by considerations going well beyond material advantage or cost enefit calculations, exercised the ultimate reponsibility for such a step, implies a further lifficulty for any extension of functionalist thery. For one thing, these results indicate that olicy formation in an industrial democracy, ven one that has corporatist politics, is not relucible to a mere interplay among organized nterests. For another, it implies that movement oward such integration cannot be achieved imply by leaving matters on a technical level vhere interest groups are a leading policy influnce. The problem is that the functionalist view nay imply the operation of a benign invisible and which will guide nations toward supranaional integration in a nonideological, depolitiized, and overwhelmingly economic setting. ut no more than in the realm of economics an the suitability of such a laissez faire process e taken for granted. In other words, what unctionalism conceives of as a "painless trancendence of the nation-state"[62] is unlikely to ome about without conscious political interention.[63]

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[58] Almost no serious assessment of the economic prospects of membership reached conclusions more pessimistic than these.

[59] This is not to imply that even fully centralized authorities can not make irrational or dysfunctional decisions.

[60] In this regard, they resembled American trade associations, which Bauer, Pool and Dexter found reluctant to take stands on issues on which unanimity was absent. (*American Business and Public Policy*, p. 337).

[61] E. g., see Leon N. Lindberg, *The Political Dynamics of European Economic Integration* (Stanford: Stanford University Press, 1963).

Moving beyond the confines of the British ase, one can generalize about the importance f the distinctly political function.[64] At least in the task of enlarging a geographic area of integration—if not in expanding that integration once a grouping already exists—a conscious political decision must first be made. Just as the political leadership of Macmillan and Wilson was essential in moving Britain to seek Common Market entry, so the intervention of Chancellor Adenauer in carrying Germany through the transformation from the European Coal and Steel Community to the EEC, and the role of President de Gaulle in vetoing British entry in 1963 were major instances of national political leadership for which functionalist theory had made no place.[65]

This perspective and that of the functionalists need not conflict totally, in that (as noted earlier in this article), the functionalists have been primarily concerned with integration within existing communities, rather than with the geographic expansion of integration or the construction of new communities. Indeed, when Britain does become a member of the EEC, circumstances will then exist in which the functionalist hypothesis is likely to apply. Major portions of functionalist theory do in fact remain valid and theoretically fruitful, but it is clear from testing the functionalist hypothesis against the British case, as well as in considering the criticisms raised by other recent writings,[66] that the functionalist theory of supranational integration would seriously overreach itself if applied to this major kind of problem. Thus at least in the case of Britain's approach to Europe, another theoretical perspective, specifically that of group politics, seems to be in order.

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[62] Brenner, *Technocratic Politics and the Functionalst Theory of European Integration,* p. 1.

[63] There is also a normative implication here. It is hat parties ought to play a greater role than pressure roups in the policy-making process. Unlike pressure roups, parties provide at least the opportunity for olitical leaders to make independent judgments on he basis of some broad interpretation of the national terest. But during the nonpoliticized phase of Euroean policy making, those intimately involved in the rocess were nonelective, not responsible to the public, nd concerned to maximize values less broad than hose of the country as a whole. More generally, the unctional representation process tends to manifest a tatic bias. It provides bargaining advantages for those terests which are better organized, while minimizing ore diffuse considerations of public benefit. See ndrew Shonfield, *Modern Capitalism* (New York: xford University Press, 1965), p. 389. For other ritical views, see Finer, *Anonymous Empire,* pp. 126–29; Sigmund Neumann, "Toward a Comparative tudy of Political Parties," in *Modern Political Parties,* d. Neumann (Chicago: University of Chicago Press. 956), p. 397; and Bernard Crick, *Observer* (London), ctober 23, 1966. But for views which question the bility of parties to promote the broader interest, see . R. Pennock, "'Responsible Government,' Separated owers, and Special Interests: Agricultural Subsidies Britain and America," *American Political Science eview,* 56 (September, 1962), 621 and 633; also Vilson, *Pressure Group,* pp. 210–211.

[64] Stanley Hoffmann's argument also seems applicable here. He has indicated the importance of an irreducible political core of "high politics," which is not susceptible to gradual erosion through step-by-step functional or spillover processes. See "European Process at Atlantic Crosspurposes," p. 92.

[65] Lindberg and Scheingold find, directly contrary to Haas in *The Uniting of Europe,* that the successful transformation leading to the establishment of the Common Market itself was not due to functional spillover: "The transformation of the European Community cannot be adequately described within the context of the standard neofunctional model with its heavy emphasis on supranational institutions and functional linkages." *Europe's Would-Be Polity,* p. 243.

[66] E. G., Lindberg and Scheingold; Brenner, *Technocratic Politics;* and Roger D. Hansen, "Regional Integration: Reflections on a Decade of Theoretical Efforts," *World Politics,* 21 (January, 1969), 242–271.

Political Clientelism and Ethnicity in Tropical Africa:* Competing Solidarities in Nation-Building

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The analysis presented here is an attempt to follow up, qualify, and elaborate upon, some earlier hypotheses about the role of political clientelism in the light of empirical data from West and Central Africa.[1] More specifically, the aim is, first, to use the concept of clientelism as an auxiliary tool to elucidate some of the ambiguities surrounding the relationship between ethnicity and nation-building in contemporary Africa; and, second, to provide what appears to be a "missing link" between the micro- and macrosociological or system-centered theories that have dominated the field of developmental politics since the last decade. Succinctly stated, this "missing link" is to be found in the varieties of personal and institutional linkages and reciprocities which, depending on the circumstances, have bridged or accentuated ethnic and other discontinuities within traditional and modernizing pluralistic polities.

Because it cuts across both "traditional" and "modern" referents, political clientelism, as a concept, has a heuristic value generally missing from the conceptual arsenal of either "modern" or "traditional" polities; it directs attention to processes of adjustment between traditional and modern patterns of behavior, expectations, and normative orientations to politics which might otherwise go unnoticed. As an analytical concept, clientelism brings into focus units of analysis, political actors, and relationships which have thus far received scant attention from political scientists.[2] As a developmental construct, it suggests new perspectives from which to look at processes of nation-building in Africa: Viewed from the micropolitical perspective of traditional patterns of interaction among groups and individuals, nation-building becomes not so much an architectonic, voluntaristic model divorced from the environmental materials available; it becomes, rather, a matter of how best to extend to the national level the discrete vertical solidarities in existence at the local or regional levels. Seen through the prism of traditional forms of clientelism, the processes of integration and disintegration that have attended the growth and decline of historic African polities may give us additional clues to similar processes in contemporary Africa. Finally, looked at from the standpoint of the exchange processes involved in clientelism, patterns of resource allocation and distribution may yet emerge as among the most meaningful indicators of political development; at least they may provide a useful corrective to the current bias toward power accumulation as a presumed prerequisite of political modernization.

This said, two cautionary remarks are in order: One is that the empirical data available are still far too limited and unreliable to permit anything like a full-fledged theory of political development; although an attempt has been made in the latter part of this discussion to relate clientelism to current theories of development, the accent here is primarily on "national integration." What follows, then, is better seen as a series of tentative and piecemeal hypotheses awaiting further testing and systematization. Moreover, if many of the observable patterns of behavior in contemporary Africa are indeed reminiscent or expressive of traditional client-patron relationships, it does not follow that *all* such patterns are reducible to clientelism.[3] This kind of naive reductionism would make unduly short shrift of the emergent integrative mechanisms, norms, and symbols attendant upon the rise of nationalist sentiment and activity, and hence ascribe to clientelsm more credit than it actually deserves.

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[1] See René Lemarchand & Keith Legg, "Political Clientelism and Development: A Preliminary Analysis," *Comparative Politics*, Vol. 4, No. 2 (Jan. 1972), 149–178.

[2] A notable exception is William J. Foltz, "Social Structure and Political Behavior of Senegalese Elites." *Yale Papers in Political Science* No. 33; see also C. S. Whitaker's illuminating discussion of "clientage" phenomena in Northern Nigeria in *The Politics of Tradition* (Princeton: Princeton University Press, 1970), pp. 373–375. Nor is this neglect typical of political scientists alone, as can readily be inferred from a content analysis of a recent issue of the *Canadian Journal of African Studies* 1 (Winter, 1969) devoted to "Rural Africa:" Clientelism is conspicuously absent from the list of research priorities assigned to various cross-disciplinary subfields (Political Anthropology, Local Politics and Development Administration, Agricultural Economics, Rural Sociology and Communications, and Rural Geography).

[3] Even where the evidence suggests the presence of clientelistic networks, these may in fact be so fluid and

Clientelism, Ethnicity, and Integration

Although it would be too much to expect universal agreement on any single definition, for the purpose of this discussion political clientelism can be viewed as "a more or less personalized relationship between actors (i.e., patrons and clients), or sets of actors, commanding unequal wealth, status or influence, based on conditional loyalties and involving mutually beneficial transactions."[4] However vague, his definition has a dual advantage. First, it is broad enough to encompass synchronic and diachronic variations (i.e., the various kinds of microlevel solidarities associated with the operaion of traditional forms of clientelism, as well as the broader, overarching solidarities characeristic of the machine pattern). Second, it suggests alternative means for integration where coercive power is not sufficiently coercive to command widespread compliance and where conceptions of legitimacy are as yet too weak or circumscribed to produce consensus.

This type of situation is clearly the rule rather than the exception in most of Africa. Equally plain is that, whatever virtues clientelsm may have in bringing people together, a more prevalent type of integration is that which has taken place at the local or regional level hrough horizontal solidarities associated with kinship or ethnic ties, or the extension of such solidarities on a more inclusive scale—in short, hrough ethnicity.[5] It is less obvious, however, that clientelism and ethnicity have seldom operated independently of each other, particularly in recent times. Just as ethnicity has sometimes been credited with integrative properties that really belong to the realm of clientelism, so clientelism as an integrative mechanism has often developed out of the exigencies of ethnic fragmentation.

Aside from the conceptual vagueness attached to each term, analysis of the relationship between clientelism and ethnicity is rendered peculiarly arduous by the similarity of the processes to which they refer. For if ethnicity is a matter of self-definition, and if "its ultimate referent is necessarily subjective,"[6] the same can be said of patron-client relationships. Thus what from one standpoint might be regarded as an example of ethnicity in the sense in which Wallerstein uses the term—a redefinition of one's cultural identity through membership in a wider community, usually in the context of an urban situation[7]—might, from another angle, be viewed as a redefinition of rural clientelistic ties in an urban setting. Moreover, clientelism and ethnicity may have overlapping memberships, with some individuals solidly anchored in the ethnic substructure, and others acting as intermediary links between this substructure and the higher reaches of the clientelistic pyramid. What may be taken for a clear example of ethnicity at one level may be nothing more than the lower reticulations of a more extensive clientelistic network. A third source of confusion lies in the reciprocal processes of interaction which normally enter into the ethnicity-clientelism relationship. Rarely has this relationship been unidirectional or fixed. Just as

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unstable as to defy analysis. Especially pertinent in his connection is Professor Young's query (in a peronal communication): "Clientelism (in the Congo) is ertainly crucial, but how can one deal with it when ll is fluidity, when the networks are far more ephemral, when mutual costs and benefits of maintenance of particular patron-client sets are apparently recalcuated very frequently, and on the basis of very short-un contingencies?" In such a spasmodic environment he analysis of clientelism is evidently a far more isky endeavor than in settings where client-patron elationships are more clearly institutionalized and offer a basis for relatively stable interactions among actors. If for no other reason, our analysis is restricted o the latter type of situation, as illustrated by Senegal, Mali, the Ivory Coast, Northern Nigeria, Rwanda, Burundi and Liberia. Among other states that might it into this category but which are not specifically dealt with in this paper one might mention the Sudan, Chad, Upper Volta, Niger, Dahomey, Sierra Leone and Mauretania.

[4] Lemarchand & Legg.

[5] The case for ethnicity as a source of national integration is stated in Immanuel Wallerstein, "Ethnicity and National Integration in West Africa," *Cahiers d'Études Africaines*, 1 (October, 1960), 129–139; using "tribalism" in a very different sense from that which Wallerstein ascribes to the term, Richard Sklar makes a similar case in "The Contribution of Tribalism to Nationalism in Western Nigeria," *Journal of Human Relations*, 8 (Spring-Summer, 1960), 407–415. Unless otherwise specified, our use of the term ethnicity incorporates Wallerstein's notions of "tribe" (a reference group defined by its loyalty to a tribal government, in a rural setting) and "ethnicity" (denoting loyalty to a community detached from its traditional government, in an urban setting). Our assumption here is that the boundaries of ethnicity are inevitably self-ascriptive, regardless of the setting—a point persuasively argued by Paul Mercier, ("On the Meaning of Tribalism in Black Africa") in P. Van der Berghe, ed., *Africa: Social Problems of Change and Conflict* (San Francisco: Chandler Publishing Co., 1965), pp. 483–501.

[6] See Aristide Zolberg, "Ethnicity and National Integration," a paper prepared for delivery at the 1966 Annual Meeting of the African Studies Association, Bloomington, Indiana, p. 2.

[7] Wallerstein.

clientelism may in some cases weaken the strength of ethnic identification, the latter may in turn affect the shape and stability of clientelistic networks.

If their contours are often blurred by the complexity of their interrelationships, clientelism and ethnicity are nonetheless analytically separable—and so are their relative contributions to national integration. The first thing to note in this connection is that although clientelism and ethnicity are both relational concepts, involving a subjective definition of the position of an individual or group in relation to another, clientelism incorporates a potentially wider range of social referents and thus offers *wider scope for integration.* Clientelism in M. G. Smith's terms, "incorporates such differentiating factors as ethnicity, occupational status, lineage, and rural-urban distinctions, and defines the boundaries of the political society . . . in functional terms"[8] or, one might add, if not always in the functional terms associated with traditional forms of clientelism, at least in terms of mutual interest. In the case of ethnicity, perceptions of mutual interest are dependent upon, and limited by, perceptions of cultural affinities; clientelism, on the other hand, extends these perceptions beyond the realm of primordial loyalties and establishes vertical links of reciprocity between ethnically or socially discrete entities.

This is not to argue that perceptions of mutual interest will necessarily prevail over ethnic loyalties and cause their extinction—though this possibility is by no means excluded. The mutations of ethnic loyalties entailed by the play of clientelism may range from extinction to dilution and accommodation, depending on the nature of the reciprocities involved and the level at which they operate.

Three possible types of situations suggest themselves: First, there may be a substitution of the client's original ethnic identity for that of his patron: A case in point is Gallais's description of the Bobo tribesman who, upon settling in Mopti (Mali), looks for a protector and having found one, becomes his client and through a kind of mimetic process, acquires not only the external behavioral patterns of his patron but his ethnic identity.[9]

Second, clientelism may cause a redefinition of one's original tribal identity by reference to a wider cultural focus. Though often conceptualized in terms of "super-tribalization"[10] or "ethnicity," much of this phenomenon is reducible to a quest for an alternative focus of security, in the form of clientelism, to that provided through clan or lineage affiliations. Thus, speaking of "associations founded by the urban tribal population" of Monrovia, Frankel notes that these associations "gain respectability from (the widespread custom of patronage), and the patrons, for their part, expect to be able to count on the support of their members for personal and political ends."[11]

Third, clientelism may provide the cement by which ethnic identities are amalgamated within the boundaries of a more inclusive political system. This phenomenon calls to mind two different types of situations: (1) those in which clientelism acts as the major "social adhesive"[12] of the traditional political system, in effect extending to the traditional political arena the principle of reciprocity operative at the level of interpersonal relations (as in the emirate system of Northern Nigeria), and (2) those in which clientelism operates within the context of a nationwide political party and takes on the qualities of machine politics (as in the case of the *Parti Démocratique de la Côte d'Ivoire* [PDCI] in the Ivory Coast). However different the processes and reciprocities involved, in each case the result has been to accommodate ethnic discontinuities to the requirements of a more or less integrated political community.

As the foregoing suggests, although clientelism and ethnicity—like integration—are relative concepts, implying varying degrees of cohesiveness and identification, clientelism denotes a *greater variety of possible types and levels of integration.*

In the perspective of the modern African state system, such integrative potentialities as ay exist in clientelism operate essentially at wo levels: Clientelism can be evaluated for its ontribution to processes of unification among ll or some of the parts of the ethnic subsys-em, or to the adaptation of these parts to the entral governmental and bureaucratic struc-ures. In either case integration stems from the xchange of valued and presumably scarce re-ources, which in turn permits the attainment f individual or collective goals. Yet the kinds f resources, exchanges, and normative expec-ations involved are liable to differ significantly rom one level to another, and so also will the nplications for national integration.

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[8] Michael G. Smith, *Government in Zazzau* (London: Oxford University Press, 1960), p. 260.

[9] Jean Gallais, "Signification du Groupe Ethnique au Mali," *L'Homme* 2 (May-August 1962), p. 122.

[10] The term "super-tribalization." suggested by Jean Rouch in "Migrations au Ghana," *Journal de la Société des Africanistes* 26, Nos. 1–2 (1956), 163–164, has more restrictive implications than Wallerstein's use of the concept of ethnicity as it suggests common cultural affinites between the group from which an individual is detached and that in which he is incorporated. For Wallerstein, ethnicity does not presuppose preexisting cultural bonds. Regardless of this distinction, the process to which these terms refer occurs on a far wider scale than that involved in ethnic mutation through the identification of a client with his patron; one might say that the latter is an individual mutation and the former a collective one usually taking place within the context of ethnic associations.

[11] Merran Fraenkel, *Tribe and Class in Monrovia* (London: Oxford University Press, 1964), p. 187.

[12] The expression is borrowed from C. S. Whitaker, *Politics of Tradition*, p. 373.

By way of illustration one might compare e operation of traditional clientelism in orthern Nigeria with the machine form oper-tive in Senegal. In his discussion of *bara*-ship Nupe, Nadel calls attention to three different ypes of integrative responses elicited through he inducements of clientelism:[13] first, the type f personal identification that originates from he desire of an inferior to obtain the political rotection of a superior; second, that which re-ults from the efforts of "impecunious crafts-en to obtain a prosperous customer"; third, hat which, "over and above securing material ssistance, stems from attachment to a man of ank and influence," thereby offering the client chance "of becoming somebody," "of being ifted from the ignominy of a commoner's life nto the sphere of rank and importance." hether the motive happens to be a quest for rotection, for material assistance, or for social obility, integration at this level stems from an xchange of psychological and material re-ards between a *superior and a subordinate;* xtended to the emirate system the relationship emains basically the same: "The crux of the lientage relationship in all the emirates," rites C. S. Whitaker, "is that patronage, eco-omic security, and protection can be ex-hanged for personal loyalty and obedience."[14]

Rather different is the type of clientelism hrough which the interests of maraboutic elites f Senegal are aggregated within the *Union rogressiste Sénégalaise* (UPS). What W. Foltz escribes as "a pyramiding of clan alliances in uper patron-client relationships" involves, in ddition to personal bonds of friendships be-ween the national elites and the maraboutic otables, certain calculated reciprocities, in the orm of policy outputs and votes, between the arty leadership and the leaders of the Muslim brotherhoods. "The prerequisite of political action," according to Foltz, is "not that it be progressive, nationalist, or such, but that the spoils be fairly distributed. The parallels with American ethnic and urban politics are fairly close; as in American cities, long range planning is often sacrificed to short-run expediency."[15] Clientelism, in short, becomes an adaptive device through which the demands of the maraboutic patrons are channeled into the political system and converted into policy outputs in return for electoral support.

In one case, integration depends on reciprocities between superiors and subordinates; its scope is limited to and at the same time delineates boundaries of the subsystem; the payments include predominantly affective rewards, in the form of identifying with a superior or receiving deference from an inferior. In the other case, integration hinges on reciprocities between equals; its scope transcends the boundaries of the ethnic or cultural subsystem; and the transactions are invested with a high degree of instrumental rationality.

If the first of our examples retrospectively suggests the nature of the obstacles placed in the way of national integration through the operation of microlevel clientelistic solidarities, the second indicates a far greater area of compatibility among the different levels at which clientelism operates. In the case of Senegal, the contribution of clientelism to nation-building would seem to involve several different types of integrative mechanisms: First, insofar as it implies a normative commitment to certain "instrumentalities and procedures for the achievement of goals and resolving conflicts,"[16] clientelism may provide at least some of the ingredients which enter into processes of consensus formation at the national level. Second, to the extent that it creates functional reciprocities among otherwise unrelated ethnic groupings, it may also generate feelings of interdependence among them and help reduce the saliency of primordial loyalties. Third, through the vertical bonds it creates among hierarchically distinct groups and communities, clientelism may supply the critical links between rulers and ruled, and through the intervention of "brokers" help bridge the proverbial gap between the elites and the masses. These three types of integration can be referred to, respectively, as "subjective," "transactional," and "structural."

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[13] Siegfried F. Nadel, *A Black Byzantium* (London: xford University Press, 1951), p. 123.

[14] Whitaker, p. 374.

[15] Foltz, "Social Structure and Political Behavior . . .," p. 153.

[16] Myron Weiner, "Political Integration and Political Development," *Annals of the American Academy of Political and Social Science,* 358 (March, 1965). 53. The distinctions drawn in this paragraph lean heavily on Weiner's classification. For a somewhat different approach, see John N. Paden, "Conceptual Dimensions of National Integration Theory, with Special Reference to Inter-Ethnic (Horizontal) Integration," Internal Working Paper No. 2, Instability/Integration Project, Council for Intersocietal Studies, Northwestern University (January, 1968).

To emphasize the integrative potential of clientelism is not to deny the divisive tendencies inherent in the formation of factions and personal followings. Nadel's comments on the effects of factionalism in Nupe might apply to other contexts as well: "Rivalry and feuds between the leaders of local factions may throw the whole town, indeed the whole country, into a civil war. . . ."[17] In specific situations, however, the mere existence of factionalism may reinforce rather than diminish popular allegiances to the system, and hence positively aid subjective integration at the national level. This is the core of the argument advanced by Gluckman in his discussion of "rebellion" (as distinct from "revolution") in a traditional monarchical context:

> All sections struggle for the kingship and this unifies them. They seek to place their own prince on the throne; they do not try to become independent from the kingship. . . . It is a historical fact that these struggles kept component sections of the nation united in [a] conflicting allegiance about the sacred kingship.[18]

Gluckman's hypothesis cannot be accepted unreservedly, if only because much of the outcome would seem to depend on the character and composition of the factions involved, and the environmental conditions under which competition takes place. Nevertheless, it does not suggest that competition among patrons could encourage the cohesiveness of different ethnic segments in the lower reaches of the clientelistic pyramid.

Finally, although clientelism and ethnicity both provide opportunities for manipulation and adaptation, clientelism offers a significantly *broader range of strategies for the achievement of national integration.* For purposes of analysis these strategies may be conceptualized in terms of (1) a pyramiding of patron-client ties, (2) a spillover of reciprocities, and (3) changing partnerships, each involving specific changes in the character and structure of clientelistic relationships. What these changes consist of, and how they may affect national integration, will be discussed at greater length in a subsequent section. Suffice it to note here that although these processes are induced and no spontaneously occurring, their viability depend in part on the environmental transformatio arising from colonial rule and on the limit tions imposed by the character of the ethni and clientelistic subsystem.

Since political integration is often a functio of the limitations and opportunities inherent i traditional clientelistic networks, it will be useful at this stage to take a brief look at some c the varieties of traditional dependency relatio ships encountered in Tropical Africa.

Varieties of Traditional Dependency Relationships

For most anthropologists, clientelism—o "clientage"—refers to a specific type of inte personal relationship, more often than not institutionalized in the form of a contractual agreement between a superior and an inferior.[19] Examples are the *bara*-ship of Nupe, the *buhak* in Rwanda, the *bugabire* in Burundi, the *oku toisha* in Ankole. Our concern here, howeve is with the generic and the recurrent rathe than the idiosyncratic or the unique. Taking a our criteria of differentiation (1) the occupancy of role sets, (2) the normative orientations of patrons and clients, (3) the types o transactions involved, and (4) the base value of differential control over resources, we fin that at least four different types of politica clientelism may be said to have guided socia and political relationships in traditional Afric —the patrimonial, the feudal, the mercantile and the "saintly" type (see typological chart).

Patrimonial clientelism is explicitly politica in character. Historically associated with th emergence of archaic state systems in the Sudanic and Interlacustrine areas, it suggests a significant expansion in scale of the original community, along with an increasing differentiatio of governmental structures. In Weber's terminology, it implies "[the transformation of th original community] into a stratum of aids t the ruler [and its dependence upon him] fo maintenance through the usufruct of land, offic fees, income in kind, salaries and henc through prebends."[20] A basic feature of thi transformation is the recruitment of client chiefs who stand in relation to the sovereig not unlike bureaucrats in relation to their superior, in the sense that "they [do] not claim thei

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[17] Nadel, *Black Byzantium,* p. 126.

[18] Max Gluckman. *Custom and Conflict in Africa* (Oxford: Basil Blackwell, 1963), pp. 45–47.

[19] For a further elaboration of this definition, se Georges Balandier, "Les Relations de Dépendenc Personnelle: Présentation du Thème," in *Cahier d'Études Africaines,* 9, 35 (1969), 345–349.

[20] Cited in Hans H. Gerth and C. Wright Mills eds., *From Max Weber: Essays in Sociology* (Nev York: Oxford University Press, 1958), p. 297.

Typology of Dependency Relationships in Traditional Africa

Type of Clientelism	Occupancy of Role Sets	Normative Orientation	Type of Transaction	Base Values of Differential Control Over Resources
. *Patrimonial* Example: Sudanic States; Interlacustrine States (Rwanda, Burundi, Ankole, Buganda)	King-chief; chief-subchief	Mutual trust and loyalty	Office (political or administrative) —service and support	Power
. *Feudal* Example: Buhake (Rwanda) Bugabire (Burundi) Okutoisha (Ankole) Bara-ship (Nupe)	Notable or freeborn noble-serf or commoner	Deference-Affection	Protection—service and prestige	Status
. *Mercantile* Example: Diula-Diatigi; diulaba-diulade; dillali-mai gida	Trader (or middleman)-associate or customer	Economic self-interest and mutual trust	Commercial exchange	Wealth
. "*Saintly*" Example: Sheikh-talibé Murabit-tlamith	Religious leader (marabout)-follower (talibe)	Benevolence-Devotion	Salvation—Obedience and Service	Sanctity

NOTE: The categories included in the above typology are adapted from the process-model developed in Lemarhand and Legg, "Political Clientelism and Development: A Preliminary Analysis," *Comparative Politics*, Vol. 4, Io. 2 (January, 1972); although the terms "patrimonial" and "feudal" are used in this article to denote a possible volutionary sequence from one type of clientelism to another, in the present context they are merely intended to rovide an analytic framework for distinguishing among types. This, however, is not meant as a counterargument) the evolutionary scheme formulated by Maquet in "Institutionalisation féodale des relations de dépendence ans quatre cultures interlacustres," *Cahiers d'Études Africaines* (1969), 402–414, in which the author suggests ne possibility of an historical movement from "patrimonial" to "feudal" clientelism.

osition by right of inheritance or by virtue of ny prior connexion with the area to which ıey were appointed."[21] The underlying values, owever, are those of mutual trust and loyalty. 'he basic transaction consists in the doling out f offices in return for administrative and polit:al benefits. The base value through which inial control over scarce resources is obtained is olitical power defined in terms of the ruler's apacity to make effective use of a coercive aparatus, but as status differences emerge in onsequence of differential success in military kills, exchange relations become increasingly ifferentiated from military ones, at which oint, in Weber's terms, "princely prerogatives ecome patrimonial in character."

Unlike the foregoing, *feudal clientelism,* in he sense in which the term is here used, is not ecessarily coextensive with the formal political ierarchy; it refers to the anthropologist's more ircumscribed definition of "clientage"—i.e., to an interpersonal bond between a superior and a subordinate, as between a freeborn noble and a commoner, involving "an exchange of protection, economic security and a position in the society in return for loyalty, obedience and service from the subordinate.[22] Often institutionalized in the form of a ceremonial gift by the patron (as in traditional Nupe, where "the offering of a turban and a sword by the patron to his henchman sealed the pact"),[23] and involving contractual elements, it partakes of the qualities of the feudal homage. Yet, as a departure from what some might consider the hall-mark of feudalism,[24] the base value through which a patron lays claim to the allegiance of his client lies in the differential social prestige and influence which goes along with one's membership in a "dominant" ethnic or status group.

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[21] Lucy Mair, *Primitive Government* (Baltimore: 'enguin Books, 1962), p. 153.

[22] Ronald Cohen, "The Dynamics of Feudalism in Bornu," *Boston University Papers on Africa,* Vol. II (Boston: Boston University Press, 1966), p. 91. For further characterizations of "feudal" clientelism, see Lucy Mair, "Clientship in East Africa," *Cahiers d'Études Africaines,* 2, 6 (1961), 315–326; Edward I. Steinhart, "Vassal and Fief in Three Interlacustrine Kingdoms," *Cahier d'Études Africaines,* 7, 25 (1967), 606–624; Jacques J. Maquet, *The Premise of Inequality in Ruanda* (London: Oxford University Press, 1961).

[23] S. F. Nadel, *Black Byzantium,* p. 126.

Mercantile clientelism, traceable to the inception of the long-distance trans-Saharan trade, in the thirteenth or fourteenth centuries, has persisted in one form or another throughout and indeed long after the Malinke diaspora. Although in this case the participants are essentially concerned with the buying, storing, transport, and selling of marketable goods, out of these commercial transactions have developed more or less personalized relationships not only between buyers and sellers but *within the trading community*. The reciprocities which exist between the *diula* traders of Mali, Guinea, or Senegal and their hosts, the *diatigi,* are duplicated by similar relationships between the long-distance Hausa traders from up-country and their "landlords" in places like Kumasi, Freetown and Ibadan, and, in Ibadan between landlords (*mai gida*) and middlemen (*dillali*).[25] These reciprocities involve, besides material benefits, a sense of mutual trust between the parties. A landlord or a *diatigi* is one who not only has material resources at his disposal but who also enjoys a certain "credit rating" fron his reputation of trustworthiness and reliabilit Although the anthropological literature dealin with this type of clientelism is scarce and th political science literature virtually nonexisten the basic principle of social interaction in thi case is unquestionably that of patron-client relationships defined in terms of reciprocal economic benefits and mutual loyalties.

The fourth type, here labeled somewhat uncritically *"saintly" clientelism,* expresses itsel most conspicuously within the context of th Sufi brotherhoods (*tariqas*) of Senegal but i also found in Mauretania, Mali, Niger, an Northern Nigeria.[26] Sometimes referred to a "maraboutism," this type of relationship emphasizes the blind submission of the *talibé* t his *sheikh* or *muqaddam.* At the root of thi dependence lies the disciple's conviction tha his salvation depends on the intercession of hi marabout, and the latter's realization that hi status in society depends on the personal devotion of his followers, as well as their contributions in kind or cash. The *sheikh-talibé* relationship among the Mourides of Senegal represen the most striking example of this type of reciprocity; but one might also cite as an example th nexus of mutual obligations which to this da links the maraboutic elites of Mauretania an their followers. Whether it expresses itself in secular or religious context, this type of clientelism still provides the basic articulation of social and political processes among individual and groups in the predominantly Islamicize areas of West Africa.

Like all classifications, the foregoing is unabashedly arbitrary. None of the above categories can be treated in isolation. As the case o prerevolutionary Rwanda suggests, the patrimonial and feudal types may coexist with an indeed reinforce each other, as seems to hav happened in the case of the mercantile and patrimonial variants at particular junctures of th evolution of the Sudanic states.[27]

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[24] For Fallers and Lombard, a feudal relationship can only obtain among equals (i.e., among nobles); a clientele relationship, on the other hand, establishes reciprocities between nobles and commoners. See Lloyd A. Fallers, *Bantu Bureaucracy* (London: Oxford University Press, 1956), and Jacques Lombard, "La Vie Politique dans une ancienne société de type féodal: Les Bariba du Dahomey," *Cahiers d'Études Africaines,* 1,3 (1960), 5–45.

[25] On the sociopolitical role of the *diula* in contemporary Bamako, see Claude Meillassoux, *Urbanization of an African Community* (Seattle and London: University of Washington Press, 1968), and his article on "The Social Structure of Modern Bamako," in *Africa,* 35, 2 (1965), 125–142. Cf. Majhemout Diop, *Histoire des classes sociales dans l'Afrique de l'Ouest* (Paris: 1971), pp. 148–152. For an excellent account of the *mai gida-dillali* relationship in contemporary West Africa, see Polly Hill, "Landlords and Brokers: A West African Trading System," *Cahiers d'Études Africaines,* 6, 23 (1966), 349–367; and her "Markets in Africa," *The Journal of Modern African Studies,* 1, 4 (1963), 441–455. See also, V. R. Dorjahn and C. Fyfe, "Landlord and Stranger: Change in Tenancy Relations in Sierra Leone," *Journal of African History,* 3, 3 (1962), 391–399. Perhaps the most illuminating discussion of this type of clientelism, from the standpoint of the political scientist, is Abner Cohen, "The Social Organization of Credit in a West African Cattle Market," *Africa,* 35, 1 (1965), 8–20. For an interesting discussion of the role of the *diatigi* in traditional African society, see in particular the contributions of J. L. Boutiller and J. L. Amselle in C. Meillassoux, ed., *The Development of Indigenous Trade and Markets in West Africa* (Oxford: 1971), pp. 240–252, 253–265.

[26] For a detailed analysis of the sociopolitical rol of the Mouride brotherhoods of Senegal, see Donal B Cruise O'Brien, *The Murids of Senegal: The Socio Economic Structure of an Islamic Order* (Ph.D Thesis, University of London, 1969). See also Luc Behrman, "The Political Influence of Muslim Brotherhoods in Senegal" (unpublished ms.), and "Th Political Significance of the Wolof Adherence t Muslim Brotherhoods in the 19th Century," i *African Historical Studies* 1 (1968), pp. 60–78. Fo a broader perspective, see J. Spencer Trimingham *The Influence of Islam upon Africa* (New York Praeger, 1968).

[27] Under what conditions a particular form of clientelism brings another into being is a question that lie beyond the scope of this paper. What seems clear, how

Moreover, although client-patron relations are here conceptualized in terms of dyadic relationships for purposes of clarity, this is not meant to exclude the possibility of middlemen or brokers. In prerevolutionary Rwanda, the chief was not only a client of the sovereign but also a broker between the sovereign and the subchief; in Senegal the *sheikh-talibé* relationship was and still is mediated by regional officers (*fode*) and assistants (*tamsirs*) who might also qualify as middlemen; in Mali the *diatigi* were almost by definition ("the stranger's master") cast in the role of brokers between the *diulas* and the host community, a function they still perform in many of the trading centers of West Africa.[28] Client-patron ties, in short, are not necessarily dyadic and unidirectional, but may involve networks of reciprocities which in turn produce reversible relationships among the parties. Depending upon their position in the society, one man's patron may act as another man's client. Finally, because of its somewhat static qualities, the above typology underplays possibilities of conflicting clientelistic solidarities, as well as the potential for conflict that may arise from the juxtaposition of vertical and horizontal solidarities. While suggesting too much in the way of an integrative potential, it may also suggest the wrong kind of potential for integration.[29]

Aside from illustrating the variety of traditional dependency relationships that might conceivably be subsumed under the rubric of political clientelism, this classification sketches out how one type of relationship can be distinguished from another, and how their projections into the political space of contemporary Africa can be analyzed. Presumably, before one can infer a set of causal relationships between traditional and modern forms of clientelism, one must ask certain preliminary questions about the variables determining each type: Who are the patrons, and who are the clients? How compatible are their status position, normative orientations, and transactional roles with conditions of rapid social change? In what ways do various types of differential control over resources "fit" with the conditions of the colonial and postcolonial situations? The answers to these questions are likely to vary not only with the heritage of specific colonial policies but with the characteristics of each type of clientelism.

Along with these differences, however, we find certain basic uniformities about how, in Africa as elsewhere, power is organized and generated. Power is not so much a commodity

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ever, is that there is no logical relationship between forms of government and types of clientelism (except where governmental and clientelistic structure merely duplicate each other, as in the case of patrimonialism). If there is any common denominator to these forms of clientelism, it is to be found, in Ronald Cohen's words, "in conditions of personal insecurity, a lack of widespread use of payment for specific purposes, lack of adequate social control outside such (clientelistic) relationships, and the inability of the kinship units to perform all the required and desired services for individuals." See Cohen, "The Dynamics of Feudalism in Bornu," p. 91. Given our ignorance, the most that can be said at this point is that the emergence of mercantile clientelism, as a historical phenomenon, has been dependent upon, as well as limited by, the growth of centralized state structures; conversely, the expansion of state structures has itself been conditioned by the economic and political resources made available through the development of regional trade circuits. For further evidence on this point, see J. Vansina, "Long-distance Trade Routes in Central Africa," *Journal of African History*, 3, 3 (1962), 375–388.

[28] The brokerage function traditionally performed by the *diatigi* is described by Charles Monteil in the following terms: *"Dans les centres soudanais les étrangers sont parqués dans un quartier spécial. Les simples passagers doivent se présenter au chef de la localité et ce chef leur assigne un hôte ou diatigi ("le maître de l'étranger"). Cet hôte est, à proprement parler, le répondant de l'étranger et celui-ci ne doit agir que par son intermédiaire. . . . Souvent par leur nombre et leur richesse, non moins que par l'importance du personnel qu'ils emploient, les étrangers sont assez puissants pour jouir d'une indépendence relative, moyennant paiement de redevances. Voire même ils arrivent à constituer un état dans l'état, et dominer les pouvoirs locaux."* Charles Monteil, "Les Empires du Mali," *Bulletin du Comité d'Etudes Historiques et Scientifiques de l'AOF*, 12, 3–4 (1929), 326. See also Félix Dubois, *Tombouctou la mystérieuse* (Paris, 1897), p. 295.

[29] A further qualifier is that the above typology only deals with highly institutionalized forms of clientelism and thus excludes societies in which clientelism, though never firmly institutionalized, did in fact play a major role in the definition of traditional power relationships. Traditional Ibo society is a case in point:

> "As in the urban communities of Boston, New Haven, and New York, political survival in an Ibo community hinged fundamentally on the leader's ability to mould and maintain a personal following. . . . In the Ibo context, as in the American, the politician acted as a broker of conflicting interests and personalities and as a provider of personal favors. His political success, in short, was a function both of his ability to resolve disputes and his generosity." (Howard E. Wolpe, "Port Harcourt: Ibo Politics in Microcosm," *The Journal of Modern African Studies*, 7 [1969], 475.)

Such noninstitutionalized forms of clientelism are obviously relevant to our discussion; their omission from our typology is only justified by the lack of relevant anthropological and historical data and the consequent difficulties involved in the task of classification and comparison.

that some just happen to possess by virtue of their privileged membership in any particular group as a relational phenomenon. Inherent in each of the clientelistic patterns noted above is a relationship of reciprocity between an individual (or group of individuals) whose influence stems from his ability to provide services, goods or values that are so desired by others as to induce them to reciprocate these gratifications in the form of alternative services, goods, and values.[30] And since the norm of reciprocity is by nature malleable and indeterminate in terms of the obligations it implies—being, in Gouldner's terms "like a kind of plastic filler, capable of being poured into the shifting crevices of social structures, serving as a kind of all-purpose moral cement"[31]—one can see why, as a role model, clientelism should have persisted over time, through adjustment and adaptation. Although social mobilization may lead to different partnerships, the role model may remain basically unaffected. The evidence on this score is indeed abundant. Far from evident, however, are the types of adjustment that have taken place over the years and the conditions that have caused one type of adjustment to occur rather than another.

Irrespective of the types of clientelism operative in the traditional society, three distinctive patterns of adjustment can be discerned: First and most prevalent has been the *spillover of clientelistic reciprocities* from one field of activity to another. Reciprocities that were once restricted to a specific type of exchange have thus led to cumulative or alternative exchanges among parties. A case in point in the transformation of "feudal" into "saintly" patron-client ties within the framework of the Muslim brotherhoods of Senegal, and the projection of both into the context of "clan politics."[32] Similarly, the patrimonial ties that once existed among the ruling aristocracy of Rwanda provided the foundation for the establishment of patron-client ties of a feudal variety between Hutu and Tutsi and for the extension of dependency relationships based on the appropriation of land to a dependency relationship based on the ownership of cattle.[33] A more recent example is the "spillover" of mercantile clientelism into the realm of local politics in Ibadan: "Of all the patrons," writes Abner Cohen, "the cattle landlords are the most powerful as they have dominated the [Hausa] Quarter politically ever since its foundation in 1916. The Chief of the Quarter has always been a cattle landlord and, since 1930, he has also been the chief of the cattle market."[34] In each of these cases a new type of relationship has been added to or substituted for a preexisting one, leading to a transactional integration at the national or regional level.

Second, clientelism can lead to a *pyramiding of client-patron ties,* and, through the recruitment of new brokers, to an expansion of local or regional reciprocities on a more inclusive scale. The appropriation of landed fiefs under the hegemony of a royal clan, accompanied by the appointment of client-chiefs, was indeed a critical feature of the twin processes of state-building and territorial aggrandizement that took place throughout the Interlacustrine zone.[35] Similarly, much of the structural integration conducted under the auspices of colonial powers can be viewed in terms of a pyramiding of patron-client ties.

Third, there may be a *substitution rather than an extension of patron-client relationships,* leading to a continuation of reciprocal exchanges but in a totally different partnership. Here again the evidence, both historical and contemporary, is considerable, though not always as conclusive as one might hope. Perhaps the most striking example of the phenomenon is offered by the Rwandese revolution of 1959–62, leading to the eviction of the Tutsi patrons and a redefinition of clientelistic partnerships within the Hutu stratum.[36]

Empirically, these types of adjustments are neither self-sustaining nor mutually reinforcing. Although the building-up of dominant parties sharing the qualities of political machines is in part traceable to the selective use of one or

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[30] The basic theoretical work on this theme is Peter M. Blau, *Exchange and Power in Social Life* (New York: John Wiley and Sons, 1967). The relevance of Blau's insights to an understanding of traditional African political structures is suggested by Polanyi's description of reciprocity patterns in precolonial Dahomey; see Karl Polanyi, *Dahomey and the Slave Trade: An Analysis of an Archaic Economy* (Seattle & London: University of Washington Press, 1966), especially pp. 33–95.

[31] Alvin W. Gouldner, "The Norm of Reciprocity: A Preliminary Statement," *American Sociological Review,* 15 (1960), 175.

[32] Foltz, "Social Structure & Political Behavior of Senegalese Elites."

[33] See Claudine Vidal, "Le Rwanda des Anthropologues ou le Fétichisme de la Vache," *Cahiers d'Études Africaines,* 9, 35 (1969), 384–401.

[34] Abner Cohen, "The Social Organization of Credit . . . ," p. 12.

[35] A very similar process appears to have taken place in Northern Nigeria; see, for example, M. G. Smith, *Government in Zazzau,* pp. 6–8.

[36] See René Lemarchand, *Rwanda and Burundi* (London: The Pall Mall Press, 1970), and "Les relations de clientèle comme agent de contestation: Le cas du Rwanda," *Civilisations,* 18, 4 (1968), 553–573; see also Yves Person, *Samori: Une Révolution Diula, Mémoires de l'IFAN,* No. 80 (Dakar, 1968); for another historical example of the same phenomenon, see Moussa Oumar Sy, "Le Dahomey: Le Coup d'Etat de 1818," *Folia Orientalia,* 6 (1965), 205–238.

more of these adjustive mechanisms, their mere occurrence is no guarantee that the end result will be the creation of a nationwide political machine. Whether operating individually or in conjunction with each other, these processes may lead to widely different types of relationships between the clientelistic substructures and the center, as well as between the constituent segments of each substructure. Just as one may conceive of different types of machines operating alongside each other, one may also envisage the possibility of the party machine being superimposed upon, and in some ways tributary of, microlevel solidarities of a traditional type. Similarly, one may conceive of a machine being wholly centered upon the party structure (as in the Ivory Coast), or, on the contrary, revolving almost exclusively around bureaucratic structures of a traditional or neotraditional type (as in Ethiopia, or in Burundi until 1967). In short, before one can generalize about the effects of these internal adjustments on the political systems of contemporary Africa, it may be useful to consider the conditions which in recent times have reinforced, hampered, or altered the operation of traditional forms of clientelism.

These conditions can best be understood by looking at (1) the processes of incorporation achieved through the imposition of colonial controls; (2) the alternative structures of opportunity made available through the introduction of new resources; and (3) the new competitive processes arising from the conjunction of (1) and (2). Only by taking each of these variables into account is it possible to gain a clear picture of the fourth and most crucial variable—ethnicity.

Patterns of Incorporation, Allocation, and Competitiveness

Difficult though it may be to generalize about the effects of colonial rule upon traditional forms of clientelism, the weight of evidence suggests important areas of compatibility between the two. At the broadest level of generalization one might argue, first, that the colonial situation has created a psychological climate eminently congenial to the maintenance of a "dependence complex"[37] among Africans. Whether or not Mannoni's depiction of the personality structure of the Malagasy can be applied across the board to Africans in general, a convincing case can be made for the view that the homage of dependence sought by European colonizers through the projection of their own "Prospero complex" onto the African scene must have fostered similar dispositions on the part of traditional patrons toward their clients.[38] Second, the conditions of rapid social change generated through various transformative influences—economic, social, and political—probably intensified the clients' need for something or somebody to depend on; the heightened feelings of insecurity released by social change, along with the resultant quest for anxiety-reducing mechanisms, have operated to reinforce the strength of client-patron relationships. Third, for reasons of practicality or opportunism, European administrators have often been very tolerant toward existing forms of clientelism, and sometimes equally receptive to the creation of new ones. Where these expressed the structure of political relations—as in the case of patrimonial clientelism—and where their suppression would have entailed greater costs than seemed justifiable or tolerable, the tendency of European officials has been to use traditional clientelistic structures as instruments for mediating and legitimizing their rule.

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[37] The expression, borrowed from Dominique O. Mannoni, *Psychologie de la Colonisation* (Paris: Editions du Seuil, 1950), carries unfortunate connotations as it suggests a cultural "hang up" of some sort or another, evenly distributed among all Africans, regardless of rank or status. I substantially agree with Professor Uphoff's objection (in a personal communication), to the effect that one is here dealing not so much with a "dependency complex" as with "more or less fruitful role relationships" (resource-exchange relationships) which a person having few resources seeks to establish. Creating a client-patron relationship is often the most fruitful. Has anybody found a "dependency complex" in a wealthy market-woman, or a powerful chief, or even a clan head? I suggest that what Europeans have observed is a function of power (or powerlessness) rather than of "Africanness." What should be emphasized here is the persistence of certain psychological orientations to politics, which often tend to outlive the specific power relationships from which they originally stemmed. Externalization of the superego, leading to its identification with an outside authority or "patron" figure, is certainly a key characteristic of clientelistic orientations, a point persuasively argued by James Scott in *Political Ideology in Malaysia* (New Haven and London: Yale University Press, 1968), p. 80 ff. For a discussion of the affective component which enters into the patron-client nexus, see Abner Cohen, *Custom and Politics in Urban Africa* (Berkeley and Los Angeles: University of California Press, 1969), p. 91.

[38] See Dominique O. Mannoni, *Psychologie de la Colonisation;* for further insights into the influence of the colonial framework upon the behavior and orientation of indigenous actors see J. P. Nettl and Roland Robertson, *International Systems and the Modernization of Societies* (New York: Basic Books, 1968), esp. pp. 63–127. The notion of "inheritance situation" discussed by the authors provides a useful analytic framework for an understanding of the persistence of clientelistic norms after independence; see in particular their discussion of "Benefactor and Beneficiary as Their Reference Groups," pp. 72–81. Their analysis of the process of the "role-taking of the benefactor by the beneficiary" evokes some of the familiar dimensions explored by Mannoni in the work cited above.

Yet if there are excellent reasons for viewing "the pyramiding of patron-client ties" as a by-product of European rule, one can easily exaggerate the extent to which traditional African societies really lent themselves to this kind of manipulation. Not only can one discern major discontinuities in the overall distribution and extensiveness of clientelistic networks in any given territory (suggesting corresponding variations in the serviceability of the incorporated units); one can also perceive fundamental differences in the degree to which European policies proved compatible with the preservation of traditional forms of clientelism.

If the first type of discontinuity may clearly be attributed to the different patterns of incorporation achieved through the more or less erratic tracing of colonial boundaries, there are several possible explanations for the differential impact of colonial policies on traditional forms of clientelism.

One such explanation lies in the hypothesis advanced by Ronald Cohen that "given a similar incorporative experience differences in traditional African political structures will create significant differences in the way these systems and the people within them adapt to the modern nation-state."[39] Another possible explanation has to do with the official policies and practices of European powers, and in particular the extent to which the effective practice of indirect rule presupposed the retention of clientelism of a feudal variety. Where the preservation of "feudal clientelism" was more or less consistently regarded as a necessary adjunct of indirect rule (as in Northern Nigeria), feudal and patrimonial forms of clientelism have evidently reinforced each other, structurally or normatively.[40] But one must also note the persistence of clientelistic patterns where indirect rule was neither regarded as legitimate nor systematically pursued, and where the social system made no allowance for the operation of feudal clientelism. Thus, drawing upon his personal experience as a former colonial administrator, Pierre Alexandre notes that in those parts of French-speaking West Africa that had a high rate of turnover among European officials, "African officials, shop clerks or cocoa brokers and catechists were, on the whole, permanent;" African "middlemen" were thus cast in the role of patrons in relation to the masses, with "clientelism cutting across tribal divisions, or, more widely, across traditional jural relationships."[41]

A third explanatory factor, operating independently of the formal assumptions underlying colonial policies, involves the variable assessments of the economic implications of client-patron relationships made by European administrators. Where traditional clientelism was found incompatible with the immediate or long-range economic objectives of the colonial authorities and accordingly done away with, as in Rwanda in the early 1950s, its suppression had profoundly destabilizing consequences for the internal cohesion of the community at large.

Economic motives may have precisely the opposite results, however, perpetuating or even accentuating the patron-client nexus at the community level. Here two different types of situations may be distinguished: One, most clearly suggested by the case of the Mouride brotherhoods of Senegal, involved a deliberate attempt by the European colonizer to draw maximum benefits from the manipulation of clientelistic relations. Although this may not have been the sole motive, the prospects of economic development offered by the marabouts' control over most of the peanut producing areas, and indeed their control of the labor force needed for agricultural development, were certainly instrumental in prompting the French to preserve by all possible means the "saintly" relationship which to this day links the *talibés* to their *sheikhs*—and in trying to establish a similar, though far less "saintly," relationship between themselves and the *sheikhs*.[42]

Recognition or reinforcement of clientelistic patterns may also occur in response to the economic demands of indigenous local communities. Meillassoux's discussion of the role of *dioula* in the Ivory Coast provides a good illustration.[43] Although they were initially denied access to their original trading areas by the French army, supposedly because of the need to set up a "commercial blockade" to facilitate the subjugation of the local communities, the *diula* traders were subsequently allowed to resume their commercial activities after their former trading partners, the Gouro, repeatedly demanded that right for them. Writing in the early sixties, Meillassoux noted:

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[39] Ronald Cohen, "Research Directions in Political Anthropology," *Canadian Journal of African Studies*, 3, 1 (1969), p. 27.

[40] In Bornu, according to Cohen, "patron-client relationships continue to be of great importance . . . and people still utilize their associations with important superiors in the emirate, not the Nigerian, social system"—a statement that might also apply, *mutatis mutandis*, to contemporary Rwanda society within the context of an all-Hutu social system, and to Burundi within what threatens to become an all-Tutsi social system. (The above quotation is from Ronald Cohen's comments (p. 105) on Elizabeth Colson's article, "Competence and Incompetence in the context of Independence," *Current Anthropology*, 8, 1–2 (1967), 92–100. For a discussion of the transformations that have affected the operation of patron-client ties in contemporary Rwanda, see René Lemarchand, "Political Instability in Africa: The Case of Rwanda and Burundi," *Civilisations* 16, 3 (1966), 319, and Lemarchand, *Rwanda and Burundi*.

[41] From Alexandre's comments on Colson's article, p. 100.

[42] Behrman, "The Political Influence of Muslim Brotherhoods in Senegal."

> Diula relations with the Gouro compared to the precolonial period are apparently the same: In addition to kola—of which they largely control the trade to-day—they buy coffee and cocoa. They even improved their position in regard to their geographical extension. . . . They are the only intermediaries at the first level in the coffee-buying process.[44]

In each of these two instances, then, economic considerations have tended not only to encourage existing clientelistic networks but also to facilitate the forging of new links between the center and the periphery. Thus, through their economic base in the Gouro milieux, the *diula* were able to capitalize upon the new opportunities opened by European commerce, and to establish themselves as intermediaries between local producers and European firms; similarly, through their economic and religious influence in the countryside, the maraboutic elites of Senegal were able to provide the crucial connecting links between the rural communities and the central governmental-bureaucratic structures.

As the foregoing makes plain, the new allocative centers set up under the colonial powers have generated new resources and linkages. How these in turn have affected African societies can best be understood by reference to (1) the type of resources generated by the colonial environment, (2) the channels through which these resources have been allocated, and (3) the transaction flows attendant upon the emergence of new reciprocities.

Taking the term "resources" in its broadest sense, as Dahl does, to describe "anything that can be used to sway the specific choices or the strategies of another individual," we discover that each of the patterns of adjustment noted earlier are reducible to the adaptation of clientelistic strategies to different resources—or, if one takes the view that clientelism is itself a resource, to different combinations of resources. "Political resources," writes Dahl, "can be pyramided in much the same way that a man who starts out in business sometimes pyramids a small investment into a large corporate empire."[45] Just as the building up of corporate and African empires can be seen as one example of pyramiding of clientelistic and other resources, the fashioning of new political entities under imperial rule may be viewed in basically the same terms.

In the latter case, however, allowance has to be made for the introduction of new resources—in the form of wealth, education, technological skills, and the like—and new types of reciprocities. The result has been a shift of emphasis away from symbolic or affective rewards to expectations of material benefits, in turn leading to a sloughing off of the "mechanical" solidarities characteristic of feudal clientelism for a type of relationship, if not always based on "organic" solidarities in Durkheim's sense, at least much closer to those associated with "machine politics." Conceptualizing this transformation in terms of different perceptions of a patron's competence, one might argue with Colson[46] that the trend in recent times has been to dissociate political competence from traditional status distinctions and to identify it with the ability to manipulate technical and political resources effectively. European patrons were sought after not only because of their political "pull" but because they controlled the technical resources and know-how through which the demands of their clients could be met; by the time African "bosses" stepped into the shoes of the European patrons, however, political competence meant in essence the ability to keep in tow a reliable clientele through the dispensation of prebends. Political parties in many instances took on the qualities of political machines.

Thus, another way of looking at political change in colonial and independent Africa is from the standpoint of the new partnerships established at the center, first between Europeans and Africans, and later among Africans. But these new partnerships did not always exclude the maintenance of traditional clientelistic solidarities at the local level, or indeed, the reinforcement of such solidarities through a "spillover" effect.

Therefore the weakness of African political machines may be due not, as some have suggested,[47] to their operating at the national rather than the local level, but to their operating at cross-purposes with local machines rendered all the more powerful by their control of traditional and modern political resources.

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[43] Claude Meillassoux, *Anthropologie Economique des Gouro de Côte d'Ivoire* (Paris: Mouton & Co., 1964), and "The Gouro-Peripheral Markets between the Forest and the Sudan," in Paul Bohannan and George Dalton, eds., *Markets in Africa* (Garden City: Anchor Books, 1965), pp. 67–92.

[44] Bohannan and Dalton, p. 86.

[45] Robert A. Dahl, *Who Governs?* (New Haven & London: Yale University Press, 1961), p. 226, 227.

[46] Colson, "Competence and Incompetence in the Context of Independence."

[47] See James C. Scott, "Corruption, Machine Politics, and Political Change," *American Political Science Review*, 63, 4 (1969), 1142–1158.

For an example of how the "spillover" of reciprocities at the local or regional level may inhibit national integration one can do no better than quote C. S. Whitaker on the organization of the NPC in Northern Nigeria:[48]

> The structure of the NPC fits conveniently into this structure of traditional [clientage] relationships in at least two important respects. First, by virtue of powers it exercised through control of the government, the party was a principal agency of patronage offices, loans, scholarships, contracts, and other opportunities sought by the upwardly mobile. This could be accomplished either directly and formally or indirectly and informally through the medium of the party or ex-party men who dominated the public boards, corporations, and commissions. Second (and of greater consequence in terms of winning mass support), the interlocking directorate of local administrative and party personnel inescapably bound humble persons to traditionally august figures in their capacity as party men. The dependency that derived from the vast network of clientage relationships inherent in the traditional society were transferred to the party. Loyalty to the NPC became a way of defraying traditional political obligations.

In the national context of Nigeria, the "traditionally august figures" of the Northern Region played the role of brokers or middlemen; poised between the national and regional cultures and communities, they became the manipulators of first- and second-order resources—i.e., of patronage offices, loans, scholarships, and contracts on the one hand, and of contacts and connections on the other.[49] Similar roles have been played, with greater or lesser degrees of success by the *ganwa* in Burundi, the *sheikhs* in Senegal, the *hassan* and *zawya* in Mauretania, the *saza* chiefs in Buganda, and some traditional chiefs in the Ivory Coast, Ghana, Niger, and Upper Volta.

The relative importance of traditional patrons in the mediating process, both before and after independence, must be seen in the light of the brokerage functions they once performed at the juncture of external and internal clientelistic channels, the latter operating within the African society and the former through the sponsorship of the European oligarchy. As brokers they were all in some degree concerned with the transmission of the same resource, namely to maintain and facilitate strategic contacts and communication with individuals holding direct access to political resources. They became, in Boissevain's words, "professional manipulators of people and information to bring about communication."[50] Following up Boissevain's thesis that "brokerage is a business [in which] a broker's *capital* consists of his personal network of relations with people, and his *credit* of what others think his capital to be," one could regard the passage from colonial to independent status as representing successive transfers to capital and credit from one set of brokers to another—presumably from "traditional" to "modern" brokers. Yet in many instances, traditional patrons (chiefs, subchiefs, landlords, etc.) managed to retain a high credit rating among their people even when their capital at the national level was low or nonexistent. Thus, in the early sixties, the traditional Tutsi chiefs of Rwanda had lost virtually all contacts with the Belgian administration; but in spite of their loss of capital, certain segments of the peasantry persisted in believing that they still enjoyed considerable influence in administrative spheres. Conversely, a transfer of political credit from the traditional patrons to the new recipients of wealth and influence did not necessarily imply a commensurate transfer of capital from one category to the other. Thus instead of speaking of "patron" and "mass" parties to characterize African political groups, it might be more useful to distinguish between different patron parties enjoying different credit ratings and capital assets in different circumstances and environments.[51]

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[48] C. S. Whitaker, *The Politics of Tradition*, p. 375.

[49] The distinction between first and second order resources is drawn from Jeremy Boissevain, "Patrons and Brokers," (unpublished ms.). According to Boissevain a broker differs from a patron in that the former has control over second resources (i.e., contacts and connections) and the latter over first order resources; although the distinction between types of resources holds important implications in the context of colonial and independent Africa, it has relatively little utility for distinguishing between patrons and brokers. A more useful discussion of brokerage functions will be found in Marc J. Swartz, "The Political Middleman," in *Local-Level Politics: Social and Cultural Perspectives*, ed. Marc J. Swartz (Chicago: Aldine, 1968), pp. 199–204.

[50] Boissevain, "Patrons and Brokers."

[51] One might, for example, compare the different ways in which recruitment and patronage have affected the fortunes of the NCNC and the NPC in Nigeria. As Wolpe's study of Port Harcourt politics so plainly indicates, the growth of the NCNC has tended to reflect "profound alterations in the structures and criteria of political recruitment." With the opening of new resources at each stage of the NCNC's historical development, a new category of elites emerged, displacing the old, and assuring that wealth and numbers became the most significant political resources. At this stage "the wealthy businessmen and traders became party 'patrons,' the petty traders and contractors, party workers (or town councillors). . . . Simultaneously, the proportion of administrators and clerical functionaries on the one hand, and of professionals, educators and journalists, on the other, declined" (Wolpe, "Port Harcourt . . ." pp. 482–483). In the

That some traditional patron parties managed to keep relatively high credit ratings in times of high demand-inflation (as in Northern Nigeria, Burundi, and even Ghana), while others did not, invites consideration of the different transaction flows generated through clientelistic structures.[52] The analysis of transaction flows between traditional patrons and clients can easily be distorted by the normative idiom in which these relationships are phrased. Where the notion of an opposition is ruled out by the nature of the relationship (as in Northern Nigeria),[53] one might all too easily assume the existence of mutually indulging transactions when in fact there is little more than grudging acquiescence on the part of clients. Yet transactions of a mutually rewarding type clearly did occur during and after the colonial period between, say, the *diula* traders of the Ivory Coast and their traditional "clients" (the Gouro); the "saintly" patrons of Senegal and their *talibés;* the traditional Emirs of Northern Nigeria and some of their subordinates. In exchange for salt, cattle and *sombe,* the Gouro gave the *diula* the kola, coffee and cocoa which enabled them to become wealthy middlemen; in exchange for a promise of salvation, marabouts received from the *talibés* the labor they needed to cultivate their fields; in return for the dispensation of psychological and material gratifications, the Fulani Emirs received deference, prestige, and services from their clients, and in time, they received the political support they needed to retain control over the provincial legislature. Because of the Emirs' control over the recruitment process, and because such control was regarded as the "natural" expression of their traditional prerogatives, "those elected became, by the same logic, the clients of the Emirs."[54]

In the case of Nigeria, restriction of the scope of mutually advantageous transactions to the northern region, coupled with the cultural differences between North and South, has undoubtedly stimulated feelings of regional particularism and even secessionist aspirations in the North. In both the Ivory Coast and Senegal, however, the expansion of reciprocities on a broad geographical and socioeconomic scale has tended to favor a similar extension of integrative bonds. The profits accumulated by the Ivoirien planters were also shared by the *diula* intermediaries and even by the chiefs as "some profited directly from the new crops and most benefited at least from the increased wealth of their kinsmen;"[55] similarly, while some of the marabouts of Senegal proved eminently useful in aggregating support for the UPS elites (in particular through the sale and distribution of party cards),[56] they also received substantial rewards from the party leadership, in hard cash as well as in the form of policy outputs. In each country national integration was achieved through a widening of clientelistic ties accompanied by mutually rewarding transactions.

Clearly, mutually rewarding exchanges at one level can create basic incompatibilities at another; but one can also conceive of mutually rewarding exchanges deteriorating over time into mutually depriving ones, with similarly dysfunctional consequences for national integration. Consider the case of Mali: Initially the *diula* community shared a close identity of interest with the Union Soudanaise (US) elites, largely because of their "opposition to European commercial policies and interests."[57] They provided the US elites with strategic contacts along the Niger, among both rural and urbanized Africans, and in turn expected their contribution to the cause of Malian nationalism to be repaid in the form of new economic opportunities for themselves and their clientele.[58] Yet by the mid-

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case of the NPC, by contrast, control over the party apparatus remained firmly in the hands of the emirs throughout the preindependence period and for some time thereafter; moreover, neither wealth nor numbers played a significant part in recruitment processes or patronage. Status, rather, was the key factor. Thus if the picture conveyed by Port Harcourt politics in the fifties reminds one of contemporary New Haven or New York, this could hardly be said of Kano or Zaria. In one case power belonged to what Dahl called the "ex-plebes," in the other to the "patricians." See Robert Dahl, *Who Governs,* pp. 11–24 and pp. 32–51.

[52] The concept of transaction flow is here used to refer to the movement of commodities, persons, or messages within and between communities. See Karl Deutsch, "Transaction Flows as Indicators of Political Cohesion" in Phillip E. Jacob & James V. Toscano, eds., *The Integration of Political Communities* (Philadelphia & New York: Lippincott & Co., 1964), pp. 75–97.

[53] See B. J. O. Dudley, "Traditionalism and Politics: A Case Study of Northern Nigeria," *Government and Opposition,* 2, 4 (1967), 516.

[54] Dudley, p. 518.

[55] Ruth Schachter Morgenthau, *Political Parties in West Africa* (Oxford: The Clarendon Press, 1964), p. 171.

[56] Donal Cruise O'Brien, "The Murids of Senegal . . .".

[57] Thomas Hodgkin & Ruth Schachter Morgenthau, "Mali," in James S. Coleman and Carl G. Rosberg, *Political Parties and National Integration in Tropical Africa* (Berkeley & Los Angeles: University of California Press, 1964), p. 223.

[58] These expectations were natural considering the tenor of the economic policies contemplated by the US leaders: "At its extraordinary congress of 1960, the *Union Soudanaise* confirmed publicly some of its leaders' long-held economic views: The party resolved to launch 'immediately and vigorously' a program of economic decolonization; to create a new economic structures . . . to develop communications, intensify agricultural production, launch new industries, stress mineral exploration. . . ." Aristide Zolberg, "Political Revival in Mali," *Africa Report,* 10, 7 (1965), 18.

'sixties, the socialist options of the Keita regime had alienated a substantial number of *diula,* and the events of 1967 further intensified their sense of disaffection toward the regime.[59] The recent history of Guinea tells a somewhat similar story: The *diula* played a crucial role in the diffusion of the PDG ideology in the late 'fifties, but as in Mali, and for much the same reasons, their opposition has since become a source of major concern for the regime.

Implicit in the foregoing is that with the opening or closing off of political resources and the proliferation of new brokerage roles, competition is likely to set in among different clientelistic networks. Although the shape of competitive patterns and their implications for national integration can only be ascertained empirically, at least two generalizations suggest themselves: one is that competition is likely to take place at different levels depending on the time period considered; another is that the rate and character of clientelistic competition tends to vary with the nature of the resources made available at any given time.

A major consequence of the opening, and subsequent exhaustion, of the political resources released through the manipulation of nationalist ideology and propaganda has been to intensify clientelistic competition between African and European patrons and in time between different sets of African patrons. Each type of competition has tended to interact with and affect the outcome of the other. The history of Rwanda, for example, clearly shows the destabilizing consequences of competitive strivings between African and European patrons upon preexisting clientelistic networks. Already in the 'twenties, as a result of "their involvement in ever-widening spheres of African life, [the White Fathers] assumed more and more the character of patrons within the existing system. . . . The Hutu accepted these new patrons, seeing in them an additional source of security."[60] The increments of security gained by Hutu clients inevitably meant a loss of social prestige for the Tutsi patrons; the adverse consequences which this implied for the application of indirect rule caused the administration to intervene and reverse the trend. By the late 'fifties and early 'sixties, however, the approach of independence raised the prospects of new political opportunities for the Hutu; and the ever-expanding involvement of the Church and the administration in the process of institutional transfer, while adding significantly to the rate of competition between Hutu and Tutsi elites, provided the Hutu elite with the resources which made possible the disengagement of the Hutu masses from the hold of their traditional patrons.

If the case of Rwanda shows an intensification of inter-African rivalries through European involvement, the history of the Ivory Coast in the late forties illustrates precisely the opposite. In Rwanda the conflict of interest between Hutu and Tutsi elites (which might be conceptualized in terms of competition between a presumptive and an incumbent set of brokers) was too fundamental not to supersede their common grievances against their European patrons; in the Ivory Coast the conflict of interest between African and European planters (also reducible to a competition between different sets of brokers) was too deep, and the economic deprivations it entailed for the Africans too widespread, not to supersede inter-African rivalries. Moreover, the concurrence of economic interests between traditional chiefs and African planters further diminished the possibilities of such rivalries.[61] This situation played directly into the hands of the nationalist elites, converting the indigenous Ivoirien planters into fervent PDCI militants, and providing at the same time the economic resources through which further support could be generated from below and new networks of clientelism activated. The significance of the contribution (economic and political) made by African planters to the cause of Ivoirien nationalism becomes all the more apparent when compared with the very different situation then confronting African planters in Ghana: "In Ghana there were no European planters and the African farmers were much less involved in nationalist activities."[62]

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[59] For an excellent discussion of Mali's socialist options under Modibo Keita, see John N. Hazard, "Marxian Socialism in Africa: The Case of Mali," *Comparative Politics,* 2, 1 (1969), 1–16. The political implications of Mali's decisions to reenter the franc zone, in 1967, are analyzed in Francis Snyder, "An Era ends in Mali," *Africa Report,* 14; 3, 4 (1969), 16–23.

[60] Alison Des Forges, "Kings without Crowns: The White Fathers in Rwanda," in Daniel McCall, Norman R. Bennett, and Jeffrey Butler, eds., *Eastern African History,* Boston University Papers on Africa, Vol. III (New York: Praeger, 1969), p. 181.

[61] Ruth Schachter Morgenthau, *Political Parties in French-Speaking West Africa,* p. 171. Equally worth emphasizing are the patron-client relationships which in many cases prevailed between migrant Mossi workers and Ivoirien planters; these relationships help explain why the Mossi minority resident in the Ivory Coast never (at least until recently) became the source of acute intergroup tensions. According to M. Dupire, *"l'attitude des immigrants Mossi envers les autochtones est faite de respect, de soumission et de crainte. Ils conservent souvent d'excellentes relations avec leurs anciens patrons, qu'ils assistent en reconnaissance des chances qu'ils leur ont offertes."* See M. Dupire, "Planteurs Autochtones et Etrangers en Basse Côte d'Ivoire," *Etudes Eburnéennes,* 8 (1960), 50.

That the rate and character of clientelistic competition have tended to reflect the nature of the resources available is again nowhere better illustrated than in Rwanda:[63] Only with the opening of new political resources, through elections and constitutional reforms, did the conflict of interest between Hutu and Tutsi elites assert itself in any structured fashion; only when they realized the futility of attaining their political goals through electoral processes did the Hutu elites actively court the support of European patrons; and it was largely through the manipulation of violence that the Hutu elites were able to aggregate a solid political clientele. If violence made security a scarce resource, clientelism in turn provided the means by which security could be obtained.

In this case, the conjunction of electoral processes and violence provided the key resources through which a traditional form of clientelism reasserted itself—albeit in somewhat modified fashion—within an ethnically homogeneous stratum. Elsewhere, however, elections, with or without violence, provided the conditions for he emergence of a different type of clientelism, dentified with dominant parties often sharing he characteristics of political machines. The rony is that the same forces that encouraged he birth of political machines also helped activate the ethnic solidarities which in many intances destroyed the machine as a form of poitical organization.

Ethnicity as an Independent Variable

This brings us to a consideration of ethnicity s an independent variable: How does ethnicity —whether taken to refer to the "givens" or "assumed givens of social existence"[64]—affect the scope, stability, and functioning of clientelistic networks? The answer is anything but clear. At least three possible types of situation suggest themselves, each in some way contradicting the other: The activation of ethnic solidarities may lead to a rupture of client-patron ties (as in Rwanda); or it may set sharp cultural or geographical limitations (or both) on the scope of clientelistic networks (as in Nigeria); or it may coexist with or indeed foster clientelistic ties on a nationwide basis (as in the Ivory Coast, Liberia, and, in a qualified sense, Senegal).

What might otherwise seem like a thoroughly confusing situation becomes somewhat more comprehensible in the light of the following caveats: (1) Although clientelism and ethnicity are in many ways interrelated, they refer to basically different levels of solidarity; whereas clientelism describes a personalized relationship, ethnicity is fundamentally a group phenomenon;[65] hence, there is no compelling reason to expect concommitant variations between ethnic and client-patron solidarities. (2) Just as group solidarities can be activated at many different levels and with varying degrees of intensity, one must also expect wide variations in the extent to which ethnic solidarities can be said to conflict with or reinforce patron-client solidarities. Whether or not the activation of solidarities in one sphere leads to a weakening of such solidarities in the other is an empirical question that can only be answered in the light of specific situations. (3) Seldom has there been a direct relationship between ethnicity and clientelism; as our previous discussion makes clear, the relationship between the two can best be seen in the light of certain intervening variables, structural and conjunctural.

These intervening variables may conveniently be analyzed under the following three major headings: (1) the nature of the structural "fit" between ethnic or cultural boundaries on the one hand, and status differences between patron and clients on the other; (2) the actor's perceptions of the relative costs and benefits involved in the maintenance of particular patron-client sets; (3) the pattern of resource allocation prevailing at any given time.

"By itself," writes M. G. Smith, "clientage is unlikely to dissolve sectional boundaries wherever differences of culture, religion or race are prominent."[66] By the same token, where cultural cleavages not only are prominent but tend to coincide with patrons and clients, these cleavages may help break up the patron-client nexus. Thus, the greater the cultural differences between patrons and clients, and the more conspicuous the social distance between them, the greater the likelihood of violent ethnic strife in conditions of rapid social mobilization.[67] Rwanda is an obvious example: Just as the spread of egalitarian ideas among Westernized Hutu elements threatened the "premise of inequality" which underlay the traditional order, the emergence of this presumptive elite, together with the support they received from the European administration and the Church, provided the conditions through which a new category of patrons was able to displace the old, and a "new" and ethnically homogenized society substituted for the old pluralistic order. Although Burundi has tended toward a similar polarization of ethnic loyalties, the greater flexibility of its stratification system and the absence of a rigorous coincidence of ethnic and status distinctions have significantly delayed ethnic conflict.

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[62] Morgenthau, p. 170. This is why the CPP in Ghana never developed the characteristics of a machine" in the sense in which the term might apply o the PDCI in the Ivory Coast. Feith's description of he CPP as a machine which "exists almost excluvely to stay in power" conveys an oversimplified view of both the CPP and machine politics and thus ails to bring out the essential differences between the CPP and the PDCI: the latter's original clientele was ot made up of "verandah boys" but of rural workers; its resource base is still overwhelmingly dependent pon a plantation economy; and its "bosses" are hemselves closely connected with the rural sector, most of them still owning large plantations. As of 969, the *Bureau Politique* and *Comité Directeur* of he PDCI comprised, respectively, five and eight lanters, and at least five of the members of the High ourt of Justice and the State Security Court were lanters. Cf. Edward Feit, "Military Coups and Potical Development: Some lessons from Ghana and igeria," *World Politics,* 20, 2 (1968), 179–194.

[63] Further evidence of this phenomenon may also be found in Wolpe, "Port Harcourt: Ibo Politics in Microcosm," esp. pp. 478–485.

[64] Clifford Geertz, "The Integrative Revolution," in Clifford Geertz, ed., *Old Societies and New States* (New York: The Free Press of Glencoe, 1963), p. 109.

[65] I am grateful to Professor Crawford Young for forcefully reminding me of this fact.

Yet, even where cultural or ethnic differences between patrons and clients might otherwise spell a lack of cohesiveness, preexistent clientelistic activities may reorient the actors' perceptions of their mutual interest so as to promote transactional integration among different ethnic segments, whether at the local or national levels. This phenomenon is perhaps best illustrated by the solidarities that have developed between the Hausa landlords of Ibadan and the predominantly Yoruba Action Group leaders of the Western Region of Nigeria; what initially began as a mutually advantageous economic transaction between cattle landlords and Yoruba butchers developed into similarly advantageous reciprocities between the Action Group leadership and the Hausa landlords:

> When during the 1950's the butchers affiliated themselves within . . . the Action Group, which was until 1962 in power in the Western Region, the Hausa landlords reacted not only by joinin the same party themselves, but also by draggin almost the whole Hausa Quarter with them i joining it, and in successive elections the Ibada Hausa gave their votes to it . . . [In return for thei votes], the party not only prevented hostile actio being taken against the cattle landlords, but eve tried to prevent individual butchers from defaulting, by exerting pressure on these to honor thei obligations, and sometimes by granting loans t those among them who did not have the cash t pay.[68]

Although integration in this case occurre partly because *mercantile clientelism* ha spilled over into the political sphere, economi interests will not everywhere and inevitably assert their primacy and lead to political compromise. Moreover, compromise may also b due to several other contributory factors. I may be due to perceptions of external threat that may submerge perceptions of local antagonism, or—as in the case discussed above—t the relative smallness of the migrant community and its consequently weak political leverage regionally or nationally.

Where no prior transactional integration exists among groups, how does the allocation o new resources affect interethnic or intercommunal relations? The answer depends in part o the character of the resources available at an given time, and on the sources of control ove these resources. Where politically significant resources tend to become the monopoly of a particular ethnic or communal group, the clientelistic solidarities arising from the exploitation o these sources may possibly reinforce ethnic o communal solidarities. Take the example of th Eastern Region of Nigeria: The displacemen of "strangers" (Sierra Leonians, Yoruba, etc. in favor, first, of Onitsha Ibo, and then Ower Ibo, has reflected basic changes in the politica significance attached to specific resources (firs education, then wealth, and finally "numbers") But it has also tended to accentuate the communal bases of clientelistic politics among Ib Moreover, in time the preeminence of "numbers" at the national level has tended to relegat the Ibo as a group to a comparatively mino position in the federal superstructure, and henc has dissipated communal differences within th Ibo community while at the same time exacerbating ethnic and regional tensions between Ib and non-Ibo.

But this is by no means the only conceivabl pattern. Where ultimate control over financi and economic resources is vested in nationall

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[66] M. G. Smith, "Institutional and Political Conditions of Pluralism," in L. Kuper and M. G. Smith, *Pluralism in Africa* (Berkeley and Los Angeles: University of California Press, 1969), p. 56.

[67] This tends to confirm the more general hypothesis developed by Dahrendorf in his theory of social conflict: The more consistent the criteria of differentiation between groups—i.e., the more they tend to define and isolate groups from each other through cumulative cleavages—the greater the likelihood of violence among them. See Ralf Dahrendorf, *Essays in the Theory of Society* (Stanford: Stanford University Press, 1968).

[68] Abner Cohen, "The Social Organization Credit . . ." pp. 16–17; see also his *Custom a Politics in Urban Africa.*

oriented politicians, the distributive capacities of the system can be used to foster new patterns of reciprocity among groups, associated with the techniques of machine politics. What is involved here is the creation of new solidarities based on expectations of concrete, short-run benefits. Although the men in charge of running the machine may occasionally bolster their authority by charisma or by coercion, they can best be thought of as political entrepreneurs. Their job is to weld together disparate ethnic segments through the allocation of prebends.

Nowhere is this type of clientelism better illustrated than by the development of the PDCI in the Ivory Coast between 1952 and 1959. According to Zolberg,

> the PDCI secured support by co-opting ethnic leaders into the organization by means of a major distributive effort, itself made possible by the sheer accident of prosperity stemming from high prices for tropical commodities during and immediately after the Korean war, a prosperity which was later bolstered by protectionist props extended by France toward its showcase colony. . . . The entire system of communications between the center and the localities took the form of ethnic channels, through official representatives such as members of the Territorial (later National) Assembly, or other forms of ethnic clientship.[69]

Although the PDCI must certainly be regarded as the prototype of the African machine, elements of the machine pattern can also be detected in the UPS of Senegal, the True Whig party of Liberia, and, to a lesser extent, the Parmehutu of Rwanda. Several other parties shared at one time or another machine characteristics—the CPP in Ghana (at least until 1960), the SLPP in Sierra Leone, and each of the three major Nigerian parties in existence until 1959. That few of these "machines" were able to maintain themselves in power raises the question of the part played by ethnicity in hastening or precipitating their demise.

Two general answers have been suggested—one related to the character of the ethnic balance in any given state, and the other to the conditions affecting the enlistment of ethnic support. Arguing from a somewhat broader perspective, James Scott hypothesizes that where a single ethnic group held a dominant position, and provided the basis for the construction of a political machine, "the excluded ethnic groups . . . demanded at the very least more regional autonomy and launched secessionist revolts in some areas. The resulting threat to the territorial integrity of the state was commonly the occasion for military takeovers"[70] Moreover, the elimination of electoral processes, according to Scott, deprives the machine of the incentives it needs to hold its clientele. In the absence of electoral pressures, expectations of payoffs are bound to decline. The bonds of cohesion which initially tied ethnic segments to the machine are thus gradually loosened, introducing possibilities of ethnic strife or secession.

Formulated in these terms, neither hypothesis is entirely convincing. Can one really vizualize (as Scott does) the ethnic map of Nigeria as being characterized by a "dominant ethnic group"? Could not the Baule of the Ivory Coast just as well qualify as a dominant ethnic group —or for that matter the Wolof of Senegal? And if one associates ethnic dominance not with numbers but with social status and power, would it not be legitimate to regard the Americo-Liberians of Liberia as "dominant"? Moreover, granting that in the American context electoral processes were instrumental in keeping political machines going, in Africa these processes have usually released those very centrifugal forces that spelled the defeat of the machine as a form of political organization. Pushing the logic of the argument a step further, one is entitled to wonder why American machines declined while electoral pressures did not. Much more relevant is Scott's contention that "the durability of the machine as a political form is maximized . . . where it is part of a larger growing economy that can afford its expensive habits, and where its bosses do not have a monopoly of coercive authority."[71] The crucial factor, however, lies in the pattern of resource allocation, in the extent to which resources (economic and political) are allowed either to move evenly from the center to the periphery, or to accrue exclusively to a particular region or ethnic group. The Ivory Coast provides the clearest example of the first pattern: except for the Agni of Sanwi, and, to a lesser extent, the Bete (two of the least favored groups), resources, whether in the form of jobs, material payoffs, sinecures of one kind or another, or social overhead capital, are fairly evenly distributed among the representatives of the various ethnic segments. This is not to deny the existence of gross disparities of income between the Ivoirien bureaucratic-planter oligarchy on the one hand and ordinary peasants on the other;[72] the only point here is that the oligarchy represents a fairly wide cross-section of the ethnic interests at stake.

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[69] Zolberg, "Ethnicity and National Integration," p. 7.

[70] Scott, "Corruption, Machine Politics and Political Change," *American Political Science Review*, 63, 4 (1969), p. 1157.

[71] Scott, p. 1158

[72] The point is persuasively argued in Basil Davidson, "The Outlook for Africa," *The Socialist Register* 1966 (New York: Monthly Review Press, 1966).

In Nigeria, by contrast, resource allocation has tended to reflect and reinforce the fragmented pattern of cultural loyalties.

> The parties themselves were part of rival business and financial structures which existed to make money for the individuals concerned and provide financial backing for the parties. . . . Each regional government has gradually acquired a number of public boards by 1959 which could be used as means of dispensing patronage to party worthies.[73]

Not only did the machines reinforce the ethnic loyalties of their clientele; electoral processes offered new opportunities for the creation of satellite machines in regions other than those in which the parent organiztion operated. The effect has been to exacerbate communal tensions so that the stability of the entire federal structure is threatened.

In short, the higher the distributive capacity of the system, the more congenial the environment to the operation of machine-like clientelism; a high distributive capacity alone, however, is not a sufficient condition to ensure the stability of the system. Even more important is the capacity of the machine to strike an adequate balance of patronage along the vertical axis of class stratification and the horizontal dimension of ethnic cleavages. An exceedingly lopsided distribution of spoils on each of these dimensions may bring disaffection, revolt, or ethnic strife.

There is yet another factor to consider, perhaps the most elusive: the social context of machine politics. Drawing upon Kilson's insight that "the principle of reciprocity refers to a fundamental aspect of the traditional or neo-traditional patterns of political action [and hence] is the most effective way (for Africans) to maximize material and prestige benefits derived from politics, as well as to minimize the losses," one might look upon African machines as the embodiment of at least one aspect of African tradition.[74] Not all African machines, however, exhibit the characteristics of tradtional reciprocity patterns; even where the latter prevail there is still considerable room for variation in the way in which tradition and modernity combine and coalesce. Leaving aside those rare instances where the machine operates in the context of a no-party state and becomes synonymous with palace politics (as in Ethiopia or Burundi between 1962 and 1968), at least two different types of machines can be distinguished: the more orthodox, PDCI-type of machine, in which patronage becomes the essential source of cohesion; and the "neo-traditional machine," such as the UPS, in which exchange processes between the center and the periphery are mediated by, and contingent upon, the operation of traditional forms of clientelism at the local level. In the latter case, traditional micro-level solidarities provide the essential linkages between the party and the masses; the machine is superimposed upon, and in some ways tributary to, the clientelistic subsystem.

Furthermore, in the more orthodox type machine, ethnic segments are incorporated through a mixture of patronage and co-optation; vertical solidarities are maintained through material inducements; the party structure defines the organization of the machine, and is coextensive with its field of operation. The neo-traditional machine, on the other hand, seeks to enlist the support of micro-level clientelistic structures through bargaining with traditional patrons who act as brokers between the party elites and the masses; vertical solidarities are maintained in part through material inducements but mainly through perpetuation of deference patterns between the brokers and their traditional clientele; moreover, the party structure is not synonymous with the traditional clientelistic substructures but adjacent to it. The party structure in Senegal, for example, "remains separate from the brotherhoods"; and if the marabouts are in no position to control the party, there are, likewise, limits to how far the party can impose its policies upon the marabouts.[75]

The integrative action patterns (what Bailey refers to as "bridge-actions")[76] through which links are established between the subsystem and the macropolitical structure are likely to vary substantially from one type of machine-clientelism to the other. In each case subordinate political actors cast their lot with the central authorities because they expect certain rewards to accrue from this relationship. In the context of the more orthodox type of political machine (like the PDCI) expectations of patronage are paramount; the securing of material benefits becomes an end in itself. For traditional brokers operating in the context of a "neo-traditional" machine, however, expectations of material payoffs are only part of a larger complex of motivations. Material incentives may only serve the purely instrumental purpose, of maximizing the symbolic or affective rewards to which the brokers consider themselves entitled. Commenting on the motives which actuate "aspiring *samba linguers*" in Senegal, Foltz notes that "money is a major goal, but primarily because it can bring with it a greater following and thus increased prestige." This characterization applies equally well to the behavior of maraboutic elites. For some the resources of the political kingdom may yield handsome dividends in cash as well as other tangible rewards; but this merely strengthens their prestige as religious leaders since, as Foltz points out, "it is popularly felt that a marabout's potential for conferring grace and ultimate salvation is reflected in his personal wealth and magnificence."[77] Here the bridge-action by which the client-patron relationship is connected to the political system is really an attempt by traditional patrons to use the resources of modern politics to reaffirm a traditional relationship.

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[73] Kenneth W. J. Post, *The Nigerian Federal Elections of 1959* (London: Oxford University Press, 1963), p. 58, The prime example of a machine-type clientelism is offered by the NCNC, between 1955 and 1959. The political fortunes of the NCNC were heavily (if not exclusively) dependent on the direct or indirect financial support it received from Marketing Board and Eastern Region Finance Corporation, (via the African Continental Bank). See Post, p. 56–58.

[74] Martin Kilson, *Political Change in a West African State* (Cambridge: Harvard University Press, 1966), p. 268.

[75] See Lucy Behrman, "The Political Influence of Muslim Brotherhoods in Senegal."

[76] Frederick G. Bailey, *Tribe, Caste and Nation: A Study of Political Activity and Political Change In Highland Orissa* (Manchester: Manchester University Press, 1960), p. 248. In contrast with the types of strategy described earlier, usually originating from the center, a "bridge-action," in the context of this discussion, refers to the motives which cause the actors in the subsystem (whether patrons or clients) to establish a new type of relationship with another system.

Not all African machines, therefore, are equally dependent upon the allocation of material resources. Whereas the PDCI and the TANU, for example, are both potentially vulnerable to what Bienen calls the "vicious circle of underdevelopment" (that is, "limited resources—weak organization—limited resources"),[78] this would seem less of a danger for a neo-traditional party machine such as the UPS, in which traditional reciprocity patterns provide alternative resources for the consolidation and manipulation of grass-roots support.

To return to the question raised earlier: the incorporation of ethnic loyalties into the framework of a political machine does not necessarily do away with the brokers' loyalty to their group of origin; it merely enlarges the scope of their loyalties at the same time that it redefines their conceptions of self-interest. In other words, the relationship between ethnicity and clientelism cannot be reduced to a zero-sum game, in which increments of clientelism augment a person's sense of self-interest in the same proportion that they diminish his loyalty to the tribe. Just as the building of modern states on the debris of the old often tends to require the adjustment of tradition to modernity, rather than the substitution of one for the other, integrative processes in Africa are more often than not dependent on a redefinition of individual conceptions of self-interest in the context of multiple group loyalties.

Political Clientelism and Development

In trying to clarify the relationship between political clientelism and ethnicity, and the relationship of both to national integration, I have only touched upon one dimension of the developmental process. It may be useful, therefore, in conclusion, to shift to certain broader aspects of political development. Since there are almost as many definitions of political development as there are of clientelism, assessments of how one relates to the other will necessarily depend on one's conceptualization of each.

Political development can be looked at in one of two ways—as synonymous with the modernization historically experienced by the West, or, following Huntington's lead, as a process of institution building "independent of, although obviously affected by, the process of modernization."[79] The first of these approaches focuses on those forces of change leading to a dispersion of power; the second on the containment of such forces for purposes of power accumulation. In one case analysis centers upon such well-known facets of the "developmental syndrome" as (1) differentiation of political roles and structures; (2) equality as the "central operative ideal" of society; (3) capacity as "not only the logical imperative of system maintenance, but also the enhanced adaptive and innovative potentialities possessed by man for the management of his environment, human and nonhuman."[80] In the other the microscope is turned on "the institutionalization of organizations and procedures" as the absolute precondition of political development.

It takes no special gift of intuition to realize that the second of these approaches is clearly more compatible with our earlier characterization of machine-clientelism, and that neither is particularly congenial to traditional forms of clientelism. Indeed, in the light of the assumption which underlies the first of these approaches, the contribution of traditional clientelism to political development might seem negligible if not counterproductive: "Political development," writes Coleman, "can be regarded as the acquisition by a political system of a *consciously-sought,* and *qualitatively new and enhanced political capacity* as manifested in the successful institutionalization of (1) *new* patterns of integration . . . and (2) *new* patterns of participation and resource distribution. . . ."[81] (my emphasis). Making due allowance for the variations noted earlier among different types of traditional clientelism, one might argue that the differentiation of political roles and structures along functional lines is hardly compatible with the dichotomous, particularistic basis of role-differentiation between patrons and clients; that the principle of equality is violated by the very definition of traditional clientelism; and that the distributive capacities of the political system are lessened rather than enhanced by the "prebendal" features of traditional clientelism and the subjective criteria it implies for allocating resources. Moreover, since traditional forms of clientelism have rarely been coextensive with the boundaries of the newly created states, "the institutionalization of political organizations and procedures" would seem to require rather different materials than those available from traditional clientelistic networks.

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[77] Foltz, "Social Structure and Political Behavior . . ."

[78] Henry Bienen, *Tanzania: Party Transformation and Economic Development* (Princeton: University Press, 1967), p. 412.

[79] Samuel P. Huntington, "Political Development and Political Decay," *World Politics,* 17, 3 (1965), 393.

[80] James S. Coleman, "Introduction" in James S. Coleman, ed., *Education and Political Development* (Princeton: Princeton University Press, 1965), p. 15.

The trouble with this line of reasoning is, first, that it so reifies traditional forms of clientelism as to deny their adaptive capacities; and second, that it overstresses the compartmentalization between "traditional" and "modern" forms of clientelism, thus ignoring the contribution that one makes to the other. Each of these points requires elaboration.

The first point has already received partial substantiation from our previous discussion: That certain forms of traditional clientelism are more adaptable than others, and may lend themselves to innovation both on a horizontal and vertical plane can be explicitly inferred from our analysis of the "spillover" and "pyramiding" processes. Suffice it to note here that some of the specific features conventionally associated with modernization can just as well be identified with the operation of traditional clientelism: the patron-client relationship may contain, in germ as it were, the elements of a further hierarchical differentiation of political roles and structures; in spite of traditional status differences between patrons and clients, the reciprocities attendant upon this relationship may lead to an equalization of wealth among the parties; and the distributive capacities of the system may be enhanced as a result of the brokerage functions performed by local patrons and the control they exercise at the subsystem level.

That there may be limits to how far traditional patrons can be relied upon to accelerate processes of modernization is equally obvious; yet the very obstacles to mass political mobilizations that may be raised by traditional clientelism can be turned into major assets in the sphere of institution building. Traditional clientelism regulates political mobilization along traditional patron-client lines and maintains a measure of cohesion among ethnic segments, meanwhile permitting the accumulation of political and economic capital at the center. To the extent that it does these things, traditional clientelism releases those very resources that are needed for the "institutionalization of organizations and procedures," while at the same time insulating the ethnic subsystem from the disruptive impact of modernization. The main dilemma in this case is that those very forces that encourage the growth of political institutions at the center may in time generate their own destruction. The more power that is accumulated at the center, the greater the potential for initiating social and economic change, and hence also the greater the chances that modernization may sap the strength of traditional clientelistic networks.

Whether power accumulation at the center leads to a decay of traditional clientelism on the periphery, or, on the contrary, provides the support necessary for its continuation, depends in part on the balance of reciprocities between the central political institutions and the clientelistic subsystem. As long as exchange processes are seen as mutually satisfying by the machine politicians at the center and the traditional brokers in the countryside, the system retains its homeostatic qualities. Preservation of the system's equilibrium, however does not necessarily lead to political development. When the power asymmetry between patrons and clients is matched by gross disparities of wealth and income, and when the resources generated through this relationship become the privilege of a sybaritic class of political entrepreneurs, the chances for political development are slim. Liberia is a prime example: Although the True Whig party represents one of the most successful efforts at institutionalization in contemporary Africa (indeed to the point where it is now regarded as too successful by the incumbent elites), Liberia is hardly a showcase of successful political development. "Economic growth has not spread its benefits evenly but has tended to make the rich richer,"[82] meanwhile keeping the populations of the rural hinterland in a state of near stagnation. Economic choices in Liberia, as elsewhere in Africa, have paid relatively little attention to conventional criteria of long-range economic and political development; the volume and quality of immediate political and economic payoffs have been the only yardsticks for evaluating the soundness of social and economic polities. Although similar criticisms have been leveled against the Ivoirien plantocracy,[83] the evidence in this case suggests a far less lopsided pattern of exchange, as well as a more conscious attempt to de-emphasize short-run benefits for long-term developmental goals.

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[81] Coleman, p. 15.

The next point is that the juxtaposition of modern and traditional value orientations at different levels of the clientelistic structure may meet the requirements of modern organizational forms at the center and yet impose serious handicaps on the system's capacity to innovate. What Powell describes as the "rural problem-solving system"[84]—whereby demands for goods and services in the rural sector are met by govenmental agencies in return for voting support—is not a very widely accepted procedure in contemporary Africa; the least that can be said is that in specific instances the social context imposes major limitations on how far the local traditional patrons can tolerate reformist moves without at the same time destroying their own bases of support. The setbacks suffered by the "rural animation" program in Senegal illustrate the point: Having noted that "determined maraboutic opposition is reserved for those governmental ventures which have more than mere technical content, those inspired by theories of socialist development, which are designed not only to improve methods of cultivation but also to bring radical changes in the social structure of rural Senegal," Donal Cruise O'Brien goes on to observe that "rural animation . . . was the worst of all these schemes from a maraboutic point of view."[85] Only to the extent that exchange processes are perceived as mutually beneficial by government officials and rural patrons is there a reasonable chance of harnessing developmental goals to the clientelistic substructure This is where the cooperative movement in Senegal, in contrast with rural animation schemes, offered genuine scope for innovation.[86]

Exchange processes are conditioned not only by perceptions of self-interest but by the overall availability of resources at the center. Thus where clientelism becomes a technique for neutralizing a political opposition absorbing at the same time the economic and financial resources needed for development, the result is evidently dysfunctional in terms of the developmental process.[87] Much, of course, depends on the overall volume of disposable resources, and the relative costs involved in the diversion of such resources. In an environment of scarcity, where the demands for jobs or sinecures become ever more pressing with every new generation of university graduates, clientelism as a preventive political technique may lose its instrumental value for the achievement of long-range development goals.

Clearly, considerably more empirical data must be assembled before the patron-client model can serve as a general instrument for assessing developmental strategies. Much of what has been said in this section, therefore, must be seen as a starting point for further investigation rather than a conclusive formulation of the relationship of clientelism to development. At least three major avenues of inquiry suggest themselves:

(1) There is, first, the problem of determining the relationship of different types of clientelism to patterns of resource allocation. How effective, for example, is the party-institutionalized type of clientelism, as compared to the traditional or neo-traditional types, in promoting what Uphoff calls "political market integration" and "increases in factor endowments?"[88] What are the intervening variables that need to be taken into account in order to assess the rela-

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[82] See "Tubman Asks for Opposition," *West Africa,* January 3, 1970.

[83] Samir Amin, *Le Développement du Capitalisme en Côte d'Ivoire* (Paris: Editions de Minuit, 1967).

[84] John D. Powell, "Peasant Society and Clientelist Politics." *American Political Science Review,* 64 (June, 1970), 411–425.

[85] O'Brien, *The Murids of Senegal . . .* .

[86] O'Brien.

[87] Absorption of opposition elements (mainly student and trade-union leaders) through clientelistic techniques has been a standard procedure in both Senegal and the Ivory Coast; how far this technique can be used without posing a major threat to development goals remains to be seen. For further information on the sources and implications of the *mouvements contestataires* in each country, see *Le Monde* (Sélection Hebdomadaire), December 18–24, 1969.

[88] See Norman T. Uphoff, "Ghana and Economic Assistance: Impetus and Ingredients for a Theory of Political Development," a paper prepared for delivery at the annual meeting of the American Political Science Association, Los Angeles, 1970.

tive advantages and disadvantages attendant upon each type of clientelism? What sorts of factors or circumstances are likely to bring about the conversion of one type of clientelism into another, and with what consequences for the political system? So far only the most speculative answers can be given to these questions.

(2) Closely related to the foregoing is the question of how new political resources are likely to affect the operation of preexisting patron-client networks, and, conversely, how new types of resources, whether defined in terms of status, wealth, authority or legitimacy, can be generated out of such networks. Passing reference must be made in this connection to the wealth of interesting insights to be gleaned from Ilchman and Uphoff's seminal work on *The Political Economy of Change:* Besides offering a much more rigorous conceptualization of "political resources" than has been attempted in this article, by emphasizing the productivity and convertibility of political resources made available through self-interested exchanges, the authors suggest new and theoretically significant perspectives from which to look at clientelistic reciprocities.[89]

(3) A third area of investigation concerns the relationship between external and internal forms of dependency. However much disagreement there may be among political scientists about the extent of economic, social, and political control exercised over their former dependencies by metropolitan powers, it is difficult not to recognize the reality of such control. How these external links of dependency may in turn affect the operation of patron-client ties at tention from students of African politics. It the domestic level has as yet received little at- may be, however, that just as the international dimensions of clientelism can help us elucidate some of its domestic manifestations, the latter may in turn give us valuable insights into what Nkrumah once referred to, not inaccurately, as "clientele sovereignty." If nothing else, some of the phenomena currently lumped together under the convenient rubric of "neo-colonialism" may thus be brought into a universe of discourse more amenable to dispassionate inquiry.[90]

In view of what remains to be done to operationalize further the concept of clientelism, some may well feel that our claim to have identified the "missing link" in current theories of development is grossly exaggerated. Some questions may also arise about the universality of the phenomenon described, or the presumed generality of its manifestations. Certainly, the conclusions one may reach about the effect of clientelism on political conflict, or the absence of conflict, are bound to reflect the nature of the empirical data from which one seeks to generalize. That something does lie between "ethnicity" and "class" in contemporary Africa is nonetheless undeniable. Thus, whether referred to as "political clientelism," "the patron-client nexus," or "dependency relationships," the kind of interpersonal relationships described in this article may yet emerge as one of the central foci of inquiry in African scholarship. My hope, in the meantime, is that it can stimulate some fresh thinking on the part of those interested in political development.

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[89] Warren E. Ilchman and Norman T. Uphoff, *The Political Economy of Change* (Berkeley and Los Angeles: University of California Press, 1969). Other discussions of political exchange that might interest social scientists dealing with clientelist politics, include: Anthony Heath, "Exchange Theory," *British Journal of Political Science,* Vol. 1, Part 1 (January 1971), 91–119, and Georges Dupré and Pierre-Philippe Rey, "Réflexions sur la pertinence d'une théorie de l'histoire des échanges," *Cahiers Internationaux de Sociologie,* 46 (Jan.-June 1969), pp. 133–162.

[90] These and other questions are tentatively dealt with in my unpublished manuscript "Political Exchange, Clientelism, and Development in Tropical Africa", a paper presented for delivery at the annual meeting of the Southern Political Science Association, Atlanta, 1970.

Patron-Client Politics and Political Change in Southeast Asia

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The analysis presented here is an effort to elaborate the patron-client model of association, developed largely by anthropologists, and to demonstrate its applicability to political action in Southeast Asia. Inasmuch as patron-client structures are not unique to Southeast Asia but are much in evidence particularly in Latin America, in Africa and in the less developed portions of Europe, the analysis may possibly have more general value for an understanding of politics in less developed nations.

Western political scientists trying to come to grips with political experience in the Third World have by and large relied on either (or both) of two models of association and conflict. One model is the horizontal, class model of conflict represented most notably by Marxist thought. It has had some value in explaining conflict within the more modern sector of colonial nations and in analyzing special cases in which rural social change has been so cataclysmic as to grind out a dispossessed, revolutionary agrarian mass. By and large, however, its overall value is dubious in the typical nonindustrial situation where most political groupings cut vertically across class lines and where even nominally class-based organizations like trade unions operate within parochial boundaries of ethnicity or religion or are simply personal vehicles. In a wider sense, too, the fact that class categories are not prominent in either oral or written political discourse in the Third World damages their *a priori* explanatory value.

The second model, and one which comes much closer to matching the "real" categories subjectively used by the people being studied, emphasizes primordial sentiments (such as ethnicity, language, and religion), rather than horizontal class ties. Being more reflective of self-identification, the primordial model naturally helps to explain the tension and conflict that increasingly occurred as these isolated, ascriptive groups came into contact and competed for power. Like the class model, however—although less well developed theoretically—the primordial model is largely a conflict model and is of great value in analyzing hostilities between more or less corporate and ascriptive cultural groupings.[1] Important as such conflict has been, it hardly begins to exhaust the political patterns of Southeast Asia and Africa, let alone Latin America. If we are to account, say, for intra-ethnic politics or for patterns of cooperation and coalition building *among* primordial groups, then the primordial model cannot provide us with much analytical leverage.

The need to develop a conceptual structure that would help explain political activity that does not depend solely on horizontal or primordial sentiments is readily apparent in Southeast Asia.[2] In the Philippines, for example, class analysis can help us understand the recurrent agrarian movements in Central Luzon (e.g., Sakdalistas and Huks) among desperate tenants and plantation laborers; but it is of little help in explaining how Magsaysay succeeded in weaning many rebels away from the Huks, or, more important, in analyzing the normal patterns of political competition between Philippine parties. In Thailand, primordial demands may help us discern the basis of dissident movements in North and Northeast Thailand, but neither primordialism nor class analysis explains the intricate pattern of the personal factions and coalitions that are at the center of oligarchic Thai politics. The almost perpetual conflicts between the central Burman state and its separatist hill peoples and minorities are indeed primordial, communal issues, but communalism is of no use in accounting for the intra-Burman struggles between factions within the Anti-Fascist People's Freedom League (AFPFL) or, later, within the military regime. Ethnicity and class do carry us far in explaining racial hostilities and intra-Chinese conflict in Malaya, but they are less helpful when it comes to intra-Malay politics or to interracial cooperation at the top of the Alliance party.[3]

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[1] Two influential anthropologists who employ this mode of analysis are: Clifford Geertz ("The Integrative Revolution," in Geertz, ed., *Old Societies and New States* [New York: The Free Press of Glencoe, 1963]); and Max Gluckman (*Custom and Conflict in Africa* [Oxford: Basil and Blackwell, 1963]).

[2] A number of political studies of Southeast Asia have dealt with factionalism or patron-client ties. The most outstanding is Carl Landé's *Leaders, Factions, and Parties: the Structure of Philippine Politics,* Monograph No. 6 (New Haven: Yale University—Southeast Asia Studies, 1964). For the Thai political system, Fred W. Riggs' *Thailand: The Modernization of a Bureaucratic Polity* (Honolulu: East-West Center Press, 1966) and David A. Wilson's *Politics in Thailand* (Ithaca: Cornell University Press, 1962) pursue a similar line of analysis; and for Burma, see Lucian W. Pye, *Politics, Personality, and Nation-Building: Burma's Search for Identity* (New Haven: Yale University Press, 1962). Some notable attempts to do comparable studies outside Southeast Asia are: Colin Leys, *Politicians and Policies: An Essay on Politics in Acholi Uganda 1962–1965* (Nairobi: East Africa Publishing House, 1967); Myron Weiner, *Party-Building in a New Nation: The Indian National Congress* (Chicago: University of Chicago Press, 1967); Paul R. Brass, *Factional Politics in an Indian State* (Berkeley and Los Angeles: University of California Press, 1965); Frederick G. Bailey, *Politics and Social Change: Orissa in 1959* (Berkeley and Los Angeles: University of California Press, 1963).

As these examples indicate, when we leave the realm of class conflict or communalism, we are likely to find ourselves in the realm of informal power groups, leadership-centered cliques and factions, and a whole panoply of more or less instrumental ties that characterize much of the political process in Southeast Asia. The structure and dynamics of such seemingly *ad hoc* groupings can, I believe, be best understood from the perspective of patron-client relations. The basic pattern is an informal cluster consisting of a power figure who is in a position to give security, inducements, or both, and his personal followers who, in return for such benefits, contribute their loyalty and personal assistance to the patron's designs. Such vertical patterns of patron-client linkages represent an important structural principle of Southeast Asian politics.

Until recently the use of patron-client analysis has been the province of anthropologists, who found it particularly useful in penetrating behind the often misleading formal arrangements in small local communities where interpersonal power relations were salient. Terms which are related to patron-client structures in the anthropological literature—including "clientelism," "dyadic contract," "personal network," "action-set"—reflect an attempt on the part of anthropologists to come to grips with the mosiac of nonprimordial divisions. Informal though such networks are, they are built, they are maintained, and they interact in ways that will permit generalization.

Although patron-client analysis provides a solid basis for comprehending the structure and dynamics of nonprimordial cleavages at the local level, its value is not limited to village studies. Nominally modern institutions such as bureaucracies and political parties in Southeast Asia are often thoroughly penetrated by informal patron-client networks that undermine the formal structure of authority. If we are to grasp why a bureaucrat's authority is likely to depend more on his personal following and extrabureaucratic connections than on his formal post, or why political parties seem more like *ad hoc* assemblages of notables together with their entourages than arenas in which established interests are aggregated, we must rely heavily on patron-client analysis. The dynamics of personal alliance networks are as crucial in the day-to-day realities of national institutions as in local politics; the main difference is simply that such networks are more elaborately disguised by formal facades in modern institutions.

In what follows, I attempt to clarify what patron-client ties are, how they affect political life, and how they may be applied to the dynamics of Southeast Asian politics. After 1) defining the nature of the patron-client link and distinguishing it from other social ties, the paper 2) discriminates among different varieties of patron-client bonds and thereby establishes some important dimensions of variation, and 3) examines both the survival of and the transformations in patron-client links in Southeast Asia since colonialism and the impact of major social changes (such as the growth of markets, the expanded role of the state, and so forth) on the content of these ties.

I. The Nature of Patron-Client Ties

The Basis and Operation of Personal Exchange. While the actual use of the terms "patron" and "client" is largely confined to the Mediterranean and Latin American areas, comparable relationships can be found in most cultures and are most strikingly present in preindustrial nations. *The patron-client relationship—an exchange relationship between roles—may be defined as a special case of dyadic (two-person) ties involving a largely instrumental friendship in which an individual of higher socioeconomic status (patron) uses his own influence and resources to provide protection or benefits, or both, for a person of lower status (client) who, for his part, reciprocates by offering general support and assistance, including personal services, to the patron.*[4]

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[3] Class as well as ethnicity is relevant to Malay-Chinese conflict, since the different economic structure of each community places them in conflict. Many a rural Malay experiences the Chinese not only as pork-eating infidels but as middlemen, money lenders, shopkeepers, etc.—as the cutting edge of the capitalist penetration of the countryside.

[4] There is an extensive anthropological literature dealing with patron-client bonds which I have relied on in constructing this definition. Some of the most useful sources are: George M. Foster, "The Dyadic Contract in Tzintzuntzan: Patron-Client Relationship," *American Anthropologist*, 65 (1963), 1280–1294; Eric Wolf, "Kinship, Friendship, and Patron-Client Relations," in Michael Banton, ed., *The Social Anthropology of Complex Societies*, Association of Applied Social Anthropology Monograph #4 (Lon-

In the reciprocity demanded by the relationship each partner provides a service that is valued by the other. Although the balance of benefits may heavily favor the patron, some reciprocity is involved, and it is this quality which, as Powell notes, distinguishes patron-client dyads from relationships of pure coercion or formal authority that also may link individuals of different status.[5] A patron may have some coercive power and he may also hold an official position of authority. But if the force or authority at his command are alone sufficient to ensure the compliance of another, he has no need of patron-client ties which require some reciprocity. Typically, then, the patron operates in a context in which community norms and sanctions and the need for clients require at least a minimum of bargaining and reciprocity; the power imbalance is not so great as to permit a pure command relationship.

Three additional distinguishing features of patron-client links, implied by the definition, merit brief elaboration: their basis in inequality, their face-to-face character, and their diffuse flexibility. All three factors are most apparent in the ties between a high-status landlord and each of his tenants or sharecroppers in a traditional agrarian economy—a relationship that serves, in a sense, as the prototype of patron-client ties.[6]

First, there is an *imbalance in exchange between the two partners which expresses and reflects the disparity in their relative wealth, power, and status.* A client, in this sense, is someone who has entered an unequal exchange relation in which he is unable to reciprocate fully. A debt of obligation binds him to the patron.[7] How does this imbalance in reciprocity arise? It is based, as Peter Blau has shown in his work, *Exchange and Power in Social Life,*[8] on the fact that the patron often is in a position to supply unilaterally goods and services which the potential client and his family need for their survival and well being. A locally dominant landlord, for example, is frequently the major source of protection, of security, of employment, of access to arable land or to education, and of food in bad times. Such services could hardly be more vital, and hence the demand for them tends to be highly inelastic; that is, an increase in their effective cost will not diminish demand proportionately. Being a monopolist, or at least an oligopolist, for critical needs, the patron is in an ideal position to demand compliance from those who wish to share in these scarce commodities.

Faced with someone who can supply or deprive him of basic wants, the potential client in theory has just four alternatives to becoming the patron's subject.[9] First he may reciprocate with a service that the patron needs badly enough to restore the balance of exchange. In special cases of religious, medical, or martial skills such reciprocation may be possible, but the resources of the client, given his position in the stratification, are normally inadequate to reestablish an equilibrium. A potential client may also try to secure the needed services elsewhere. If the need for clients is especially great, and if there is stiff competition among patron-suppliers, the cost of patron-controlled services will be less.[10] In most agrarian settings, substantial local autonomy tends to favor the growth of local power monopolies by officials or landed gentry. A third possibility is that clients may coerce the patron into providing services. Although the eventuality that his clients might turn on him may prompt a patron to meet at least the minimum normative standards of exchange,[11] the patron's local power and the ab-

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don: Tavistock Publications, 1966), pp. 1–22; J. Campbell, *Honour, Family, and Patronage* (Oxford: Clarendon Press, 1964); John Duncan Powell, "Peasant Society . . . ," p. 412, Carl Landé, *Leaders, Factions and Parties* . . . ," Alex Weingrod, "Patrons, Patronage, and Political Parties," *Comparative Studies in Society and History,* 10 (July, 1968), pp. 1142–1158.

[5] Powell, "Peasant Society and Clientelist Politics," *American Political Science Review,* 64 (June, 1970), 412.

[6] Another comparable model, of course, is the lord-vassal link of high feudalism, except in this relationship the mutual rights and obligations were of an almost formal, contractual nature. Most patron-client ties we will discuss involve tacit, even diffuse standards of reciprocity. Cf. Ruston Coulborn, ed., *Feudalism in History,* (Princeton: Princeton University Press, 1956).

[7] In most communities this sense of obligation is a strong moral force, backed by informal community sanctions that help bind the client to the patron. A good account of how such feelings of debt reinforce social bonds in the Philippines is Frank Lynch's description of *utang na loob* in *Four Readings in Philippine Values,* Institute of Philippine Culture Papers, No. 2 (Quezon City: Aleneo de Manila Press, 1964).

[8] Peter M. Blau, *Exchange and Power in Social Life* (New York: Wiley, 1964), pp. 21–22. Blau's discussion of unbalanced exchange and the disparities in power and deference such imbalance fosters is directly relevant to the basis of patron-client relationships.

[9] These general alternatives are deduced by Blau (p. 118) and are intended to be exhaustive.

[10] Later, we will examine certain conditions under which this may actually occur.

[11] There is little doubt that this last resort usually acts as a brake on oppression. The proximate causes for many peasant uprisings in medieval Europe during hard times often involved revocation of small rights granted serfs by their lords—e.g., gleaning rights, use of the commons for pasturage, hunting and fishing privileges, reduction of dues in bad crop years—rights which offered a margin of security. Such re-

sence of autonomous organization among his clients make this unlikely. Finally, clients can theoretically do without a patron's services altogether. This alternative is remote, given the patron's control over vital services such as protection, land, and employment.

Affiliating with a patron is neither a purely coerced decision nor is it the result of unrestricted choice. Exactly where a particular patron-client dyad falls on the continuum depends on the four factors mentioned. If the client has highly valued services to reciprocate with, if he can choose among competing patrons, if force is available to him, or if he can manage without the patron's help—then the balance will be more nearly equal. But if, as is generally the case, the client has few coercive or exchange resources to bring to bear against a monopolist-patron whose services he desperately needs, the dyad is more nearly a coercive one.[12]

The degree of compliance a client gives his patron is a direct function of the degree of imbalance in the exchange relationship—of how dependent the client is on his patron's services. An imbalance thus creates a sense of debt or obligation on the client's part *so long as it meets his basic subsistence needs* and represents, for the patron, a " 'store of value'—social credit that . . . (the patron) can draw on to obtain advantages at a later time."[13] The patron's domination of needed services, enabling him to build up savings of deference and compliance which enhance his status, and represents a capacity for mobilizing a group of supporters when he cares to. The larger a patron's clientele and the more dependent on him they are, the greater his latent capacity to organize group action. In the typical agrarian patron-client setting this capacity to mobilize a following is crucial in the competition among patrons for regional preeminence. As Blau describes the general situation,

> The high-status members furnish instrumental assistance to the low-status ones in exchange for their respect and compliance, which help the high-status members in their competition for a dominant position in the group.[14]

A second distinguishing feature of the patron-client dyad is the *face-to-face,* personal quality of the relationship. The continuing pattern of reciprocity that establishes and solidifies a patron-client bond often creates trust and affection between the partners. When a client needs a small loan or someone to intercede for him with the authorities, he knows he can rely on his patron; the patron knows, in turn, that "his men" will assist him in his designs when he needs them.[15] Furthermore, the mutual expectations of the partners are backed by community values and ritual.

In most contexts the affection and obligation invested in this tie between nonrelatives is expressed by the use of terms of address between partners that are normally reserved for close kin. The tradition of choosing godparents in Catholic nations is often used by a family to create a fictive kinship tie with a patron—the godfather thereby becoming like a brother to the parents.[16] Whether the model of obligation established is father-son, uncle-nephew, or elder-younger brother, the intention is similar: to establish as firm a bond of affection and loyalty as that between close relatives. Thus while a patron and client are very definitely alive to the instrumental benefits of their association, it is not simply a neutral link of mutual advantage. On the contrary, it is often a durable bond of genuine mutual devotion that can survive severe testing.

The face-to-face quality of the patron-client dyad, as well as the size of the patron's resource base, limits the number of direct active ties a single patron can have.[17] Even with vast

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volts, even though they generally failed, served as an object lesson to neighboring patrons. Cf. Friedrich Engels, *The Peasant War in Germany* (New York: International Publishers, 1966); Norman Cohn, *The Pursuit of the Millennium* (New York: Harper, 1961); and E. B. Hobsbawm, *Primitive Rebels* (New York: Norton, 1959).

[12] Blau, *Exchange and Power in Social Life,* pp. 119–120, makes this point somewhat differently: "The degree of dependence of individuals on a person who supplies valued services is a function of the difference between their value and that of the second best alternative open to them." The patron may, of course, be dependent himself on having a large number of clients, but his dependence upon any *one* client is much less than the dependence of any one client upon him. In this sense the total dependence of patron and client are similar, but almost all the client's dependence is focused on one individual, whereas the patron's dependence is thinly spread (like that of an insurance company—Blau, p. 137) across many clients. Cf. Godfrey and Monica Wilson, *The Analysis of Social Change; Based on Observations in Central Africa* (Cambridge: The University Press, 1945), pp. 28, 40.

[13] Blau, p. 269.

[14] Blau, p. 127.

[15] The classic analysis of the functions of gift-giving (prestation) in creating alliances, demonstrating superiority, and renewing obligations, is Marcel Mauss, *The Gift: Forms and Functions of Exchange in Archaic Societies* (Glencoe, Illinois: The Free Press of Glencoe, 1954).

[16] See Sidney Mintz and Eric Wolf, "An Analysis of Ritual Co-Parenthood (Compadrazgo)," Southwestern Journal of Anthropology 6 (Winter, 1948) pp. 425–437.

[17] Carl Landé, "Networks and Groups in Southeast Asia: Some Observations on the Group Theory of Politics," Unpublished manuscript (March, 1970), p. 6.

resources, the personal contact and friendship built into the link make it highly unlikely that an active clientele could exceed, say, one hundred persons. The total following of a given patron may be much larger than this, but normally all except 20–30 clients would be linked to the patron through intermediaries. Since we are dealing with positive emotional ties (the ratio of "calculation" to affection may of course vary), a leader and his immediate entourage will be comparatively small.

The third distinctive quality of patron-client ties, one that reflects the affection involved, is that they are *diffuse, "whole-person" relationships rather than explicit, impersonal-contract bonds.* A landlord may, for example, have a client who is connected to him by tenancy, friendship, past exchanges of services, the past tie of the client's father to his father, and ritual coparenthood. Such a strong "multiplex" relation, as Adrian Mayer terms it,[18] covers a wide range of potential exchanges. The patron may very well ask the client's help in preparing a wedding, in winning an election campaign, or in finding out what his local rivals are up to; the client may approach the patron for help in paying his son's tuition, in filling out government forms, or in getting food or medicine when he falls on bad times. The link, then, is a very flexible one in which the needs and resources of the partners, and hence the nature of the exchange, may vary widely over time. Unlike explicit contractual relations, the very diffuseness of the patron-client linkage contributes to its survival even during rapid social change—it tends to persist so long as the two partners have something to offer one another.[19] Just as two brothers may assist each other in a host of ways, patron-client partners have a relationship that may also be invoked for almost any purpose; the chief differences are the greater calculation of benefits and the inequality that typifies patron-client exchange.

The Distinctiveness of the Patron. The role of patron ought to be distinguished from such role designations as "broker," "middleman," or "boss" with which it is sometimes confounded. Acting as a "broker" or "middleman"—terms which I shall use interchangeably—means serving as an intermediary to arrange an exchange or transfer between two parties who are not in direct contact. The role of middleman, then, involves a three party exchange in which the middleman functions as an agent and does not himself control the thing transferred. A patron, by contrast, is part of a two-person exchange and operates with resources he himself owns or directly controls.[20] Finally, the terms "middleman" and "broker" do not specify the relative status of the actor to others in the transaction, while a patron is by definition of superior rank to his client.

Important as this distinction is, it is easily lost sight of for two reasons. First, it is not always a simple task to determine if someone personally controls the resources he uses to advance himself. What of the case in which a civil servant distributed the subordinate posts in his jurisdiction to create an entourage? Here it would seem that he was acting as a patron, inasmuch as the jobs he gave out were meant as *personal* gifts from the store of scarce values he controlled and were intended to create a feeling of personal debt and obligation among recipients. The social assessment of the nature of the gift is thus crucial. If we were to find, on the other hand, that the civil servant was viewed as someone who had acted as an agent of job-seekers and put them in touch with a politician who controlled the jobs, then he would be acting as a broker. It is only natural that many an ambitious public official will seek to misrepresent acts of brokerage or simple adherence to the rules as personal acts of patronage, thereby building his following.[21] To the extent that he succeeds in representing his act as a personal act of generosity, he will call forth that sense of personal obligation that will bind his subordinates to him as clients.[22]

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[18] Adrian C. Mayer, "The Significance of Quasi-Groups in the Study of Complex Societies," in Michael Banton, ed., *The Social Anthropology of Complex Societies,* pp. 97–122. Mayer would call a short-term, contractual interaction that was limited in scope a *simplex* tie.

[19] In another sense the patron-client dyad is fragile. Since it is a diffuse, noncontractual bond, each partner is continually on guard against the possibility that the other will make excessive demands on him, thus exploiting the friendship. A patron may, for example, prefer to hire an outsider for an important job because he can then contractually insist that the work be of top quality. With a client, it would be a delicate matter to criticize the work. As in friendship, "the diffuseness of the [patron-client] obligation places a corresponding demand for self-restraint on the parties if the relationship is to be maintained." William A. Gamson, *Power and Discontent* (Homewood, Illinois: Dorsey, 1968), p. 167.

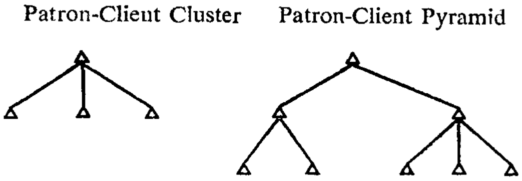
[20] A *broker* does, in a real sense *have a resource: namely, connections.* That is, the broker's power—his capacity to help people—is predicated on his ties with third parties.

[21] U.S. Congressmen spend a good portion of their time trying to seize personal credit for decisions which benefit their constituents whether or not they had anything to do with the decision—as broker or patron. For similar reasons, cabinet ministers in Malaysia and elsewhere have travelled about the country with government checks in hand, making grants to mosques, temples, and charitable groups in a way that will dramatize the largesse as an act of personal patronage. Every government decision that benefits someone represents an opportunity for someone to use that act to enlarge the circle of those personally obligated to him.

A second potential source of confusion in this distinction is that the terms designate roles and not persons, and thus it is quite possible for a single individual to act both as a broker and a patron. Such a role combination is not only possible, but is empirically quite common. When a local landowning patron, for example, becomes the head of his village's political party he is likely to become the middleman between many villagers and the resources controlled by higher party officials. In this case he may have clients for whom he also serves as broker. The diffuse claims of the patron-client tie actually make it normal for the patron to act as a broker for his clients when they must deal with powerful third parties—much as the patron saint in folk Catholicism who directly helps his devotees while also acting as their broker with the Lord.[23] If on the other hand, the political party simply gives the local patron direct control of its programs and grants in the area, it thereby enhances his resources for becoming a patron on a larger scale and eliminates the need for brokerage.

Patrons ought finally to be differentiated from other partly related terms for leadership such as "boss," "caudillo," or "cacique." "Boss" is a designation at once vague and richly connotative. Although a boss may often function as a patron, the term itself implies (a) that he is the most powerful man in the arena and (b) that his power rests more on the inducements and sanctions at his disposal than on affection or status. As distinct from a patron who may or may not be the supreme local leader and whose leadership rests at least partly on rank and affection, the boss is a secular leader *par excellence* who depends almost entirely on palpable inducements and threats to move people. As we shall show later, a settled agrarian environment with a recognized status hierarchy is a typical setting for leadership by patrons, while a more mobile, egalitarian environment is a typical setting for the rise of bosses. The final two terms, "caudillo" and "cacique" are most commonly used in Latin America to designate the regional—often rural—bosses. Again the implication is that coercion is a main pillar of power, and in the case of the *caudillo,* a personal following is common.[24] A "cacique" or "caudillo" may act as patron to a number of clients but he typically relies too heavily on force and lacks the traditional legitimacy to function mainly as a patron. At best, a *cacique* or *caudillo* may, like a boss, be a marginal special case of a patron, providing that a portion of his following is beneath him socially and bound to him in part by affective ties. Over time, however, a metamorphosis may occur. Just as the successful brewery owner of late 18th century England might well anticipate a peerage for his son, the *cacique* who today imposes his rule by force may do well enough to set his son up as a landowner, whose high status and legitimacy strengthens his role as patron.

Patron and Clients as Distinctive Groupings. To this point, the discussion has centered on the nature of the single link between patron and client. If we are to broaden the analysis to include the larger structures that are related by the joining of many such links, a few new terms must be introduced. First, when we speak of a patron's immediate following—those clients who are directly tied to him—we will refer to a patron-client *cluster.* A second term, enlarging on the cluster but still focusing on one person and his vertical links is the patron-client *pyramid.* This is simply a vertical extension downward of the cluster in which linkages are introduced beyond the first-order.[25] Below are typical representations of such links.



Patron-Client Cluster

Patron-Client Pyramid

Although vertical ties are our main concern, we will occasionally want to analyze *horizontal dyadic ties,* say, between two patrons of comparable standing who have made an alliance. Such alliances often form the basis of *factional systems* in local politics. Finally, patron-client

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[22] And it naturally follows that in underdeveloped countries, where the patrimonial view of office is especially strong, a public post could be a client-creating resource.

[23] Foster, "The Dyadic Contract in Tzintzuntzan," pp. 1280–1294.

[24] For good descriptions of both types of leadership, see Eric R. Wolf and E. C. Hansen, "Caudillo Politics: A Structural Analysis," *Comparative Studies in Society and History,* 9, 2 (January, 1967), 168–179 and Paul Friedrich, "The Legitimacy of a Cacique," in Marc J. Swartz, ed., *Local Level Politics: Social and Cultural Perspectives* (Chicago: Aldine 1968), pp. 243–269.

[25] The terms "cluster," "network," and "first" and "second" orders are adapted from a somewhat similar usage by J. A. Barnes, "Networks and Political Processes," in Swartz, ed., pp. 107–130.

networks are not ego-focused but refer to the overall pattern of patron-client linkages (plus horizontal patron alliances) joining the actors n a given area or community.

Patron-client clusters are one of a number of ways in which people who are not close kin come to be associated. Most alternative forms of association involve organizing around categorical ties, both traditional—such as ethnicity, religion, or caste—and modern—such as occupation or class—which produce groups that are fundamentally different in structure and dynamics. The special character of patron-client clusters stems, I believe, from the fact that, unike categorically-based organizations, such clusters a) have a basis of *membership that is specific to each link,*[26] and b) are *based on individual ties to a leader* rather than on shared characteristics or horizontal ties among followers.

Some other important distinctions between categorical and patron-client groupings follow from these particular principles of organization. Here I rely heavily on Carl Landé's more elaborate comparisons between dyadic followngs and categorical groups.[27]

1. *Members' Goals:* Clients have particularistic goals which depend on their personal ties to the leader, whereas categorical group members have common goals that derive from shared characteristics which distinguish them from members of other such groups.
2. *Autonomy of Leadership:* A patron has wide autonomy in making alliances and policy decisions as long as he provides for the basic material welfare of his clients, whereas the leader of a categorical group must generally respect the *collective* interest of the group he leads.
3. *Stability of Group:* A patron-client cluster, being based on particularistic vertical links, is highly dependent on its leader's skills and tends to flourish or disintegrate depending on the resources of the leader and the satisfaction of individual client demands. A categorical group, by contrast, is rooted more firmly in horizontally shared qualities and is thus less dependent for its survival on the quality of its leadership and more durable in its pursuit of broader, collective (often policy) interests.
4. *Composition of Group:* Patron-client clusters, because of the way they are created, are likely to be more heterogeneous in class composition than categorical groups which are based on some distinctive quality which members share. *By definition,* patron-client pyramids join people of different status rankings while categorical groups *may or may not* be homogeneous in status.
5. *Corporateness of Group:* In a real sense a patron-client cluster is not a group at all but rather an "action-set" that exists because of the vertical links to a common leader—links which the leader may activate in whole or in part.[28] Followers are commonly not linked directly to one another and may, in fact, be unknown to each other. An organized categorical group, by contrast, is likely to have horizontal links that join members together so that it is possible to talk of a group existence independent of the leader.

Although this listing is not exhaustive, it does illustrate the special character of patron-client networks. Bearing in mind the generic qualities of these ties, we now turn to the range of variation within the genus.

II. Variation in Patron-Client Ties

One could potentially make almost limitless distinctions among patron-client relationships. The dimensions of variation considered here are selected because they seem particularly relevant to our analytical goal of assessing the central changes in such ties within Southeast Asia. Similar distinctions should be germane to the analysis of other preindustrial nations as well.

The Resource Base of Patronage. A potential patron assembles clients on the basis of his ability to assist them. For his investment of assets, the patron expects a return in human resources—in the form of the strength of obligation and the number of clients obligated to him. The resource base or nature of the assets a patron has at his disposal can vary widely. One useful basis for distinguishing among resources is the directness with which they are controlled. Patrons may, in this sense, rely on a) their own knowledge and skills, b) direct control of personal real property, or c) indirect control of the property or authority of others (often the public). The resources of *skill and knowledge* are most recognizable in the roles of lawyer, doctor, *literatus,* local military chief, teacher—religious or secular. Those equipped with these skills control scarce resources than can enhance the social status, health, or material well being of another. Inasmuch as such resources rest on knowledge, they are less perishable than more material sources—although the *time* of the expert *is* limited—and can be used again and again without being diminished. Such resources are relatively, but not entirely, secure. In the case of lawyers and literati, for example, the exchange price of their services depends respectively on the continued existence of a court system and the veneration of a particular literary tradition, both of which are subject to change. The value of a local military chief's protection is similarly vulnerable to devaluation once the nation state has established local law and order.

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[26] Mayer, "The Significance of Quasi-Groups . . ." p. 109.

[27] Landé, "Networks and Groups . . ." (unpublished manuscript), pp. 6–12.

[28] Mayer, p. 110.

Reliance on *direct control of real property* is a second common means of building a clientele. Traditionally, the typical patron controlled scarce land. Those he permitted to farm it as sharecroppers or tenants became permanently obligated to him for providing the means of their subsistence. Any businessman is in a similar position; as the owner of a tobacco factory, a rice mill, or a small store he is able to obligate many of those of lower status whom he employs, to whom he extends credit, or with whom he does business. This kind of resource, in general, is more perishable than personal skills. A landlord has only so many arable acres, a businessman only so many jobs, a shopkeeper so much ready cash, and each must carefully invest those resources to bring the maximum return. Like any real property, moreover, private real property is subject to seizure or restrictions on its use.

A third resource base available to the potential patron is what might be called *indirect, office-based property*. Here we refer to patrons who build a clientele on the strength of their freedom to dispense rewards placed in their trust by some third party (parties). A village headman who uses his authority over the distribution of communal land to the poor or the distribution of *corvée* labor and taxation burdens in order to extend his *personal* clientele would be a typical example of traditional office-based patronship. One can classify similarly office-holders in colonial or contemporary settings whose discretionary powers over employment, promotion, assistance, welfare, licensing, permits, and other scarce values can serve as the basis of a network of personally obligated followers. Politicians and administrators who exploit their office in this way to reward clients while violating the formal norms of public conduct are, of course, acting corruptly. Finally, we should add private-sector office-holders in the private sector such as plantation managers purchasing agents, and hiring bosses, who may also use their discretionary authority to nurture a clientele.

Indirect, office-based property is least secure in many respects, as its availability depends on continuity in a position that is ultimately given or withdrawn by third parties. A landlord wil usually retain his local base whereas an office holder is likely to be swept out by a new victor at the polls or simply by a power struggle within the ruling group. In spite of the risks involved, these posts are attractive because the resources connected with many of them are far greater than those which an individual can amass directly.

The categories of resources just discussed are not mutually exclusive. It is common, for example, for a patron to have a client who is obligated to him by being a tenant on his land and also by having secured an agricultural loan through his patron's chairmanship of the ruling party's local branch. The resources that cement a dyadic tie may thus be multiple—it is often a question of deciding which is the predominant resource. Much the same analysis can be made of a patron-client cluster or network, since a patron may have clients who are bound to him by quite different resources, and it is often important to determine what the main resource is that holds the cluster together.

Resource Base of Clientage. As the other member of a reciprocating pair, the client is called upon to provide assistance and services when the patron requires them. The variation in the nature of such assistance is another means of distinguishing one patron-client dyad from another. Here one might want to differentiate: (1) *labor sevices and economic support,* as provided by a rent-paying tenant or employee, (2) *military or fighting duties,* such as those performed by members of a bandit group for their chief, and (3) *political services* such as canvassing or otherwise acting as an agent of a politician. Within the "political service" category one may wish to separate electoral services from nonelectoral political help. I should add here that the term "clients" can refer to those who are in the middle of a patron-client pyramid—being a client to someone higher up and a patron to those below. In this case, a superior patron will be interested in his client's potential services, but those services will include the size, skills, assets, and status of the client's own subordinate following.

Just as a patron-client dyad can be distinguished by the main resource base of clientage so can a patron-client cluster be categorized by the modal pattern of client services for the cluster or pyramid as a whole.

Balance of Affective and Instrumental Ties. By definition, instrumental ties play a major role in the patron-client dyad. It is nonetheless possible to classify such dyads by the extent to which affective bonds are also involved in the relationship. At one end of this continuum one might place patron-client bonds which, in addition to their instrumental character, are reinforced by affective links growing, say, from the patron and the client having been schoolmates, coming from the same village, being distant relatives, or simply from mutual love. Comparable affective rewards may also spring from the exchange of deference on the one hand and *noblesse oblige* on the other in a settled agrarian status network—rewards that have value beyond the material exchanges they often involve.[29] At the other end of the spectrum lies a dyadic tie much closer to an almost neutral exchange of goods and services. The more purely coercive the relationship is and the less traditional legitimacy it has, the more likely that affective bonds will be minimal.

This distinction has obvious analytical value. If we were to look at a patron's entire following, we would be able to classify each vertical bond acrording to the ratio of affective to instrumental rewards involved. (one could, of course, do the same for horizontal alliances. Using this criterion we could identify a set of followers among whom the ratio of affective to instrumental ties was relatively high, reflecting perhaps distant kinship, old village or neighborhood ties, or comparable bonds. The loyalty of this set of followers would be less dependent upon a continued flow of material benefits, simply because their loyalty is partly based on nonmaterial exchanges. As we move beyond this partly affective following to a patron's other supporters, the weight of instrumental, usually material, ties becomes relatively more important. The nature of a man's following—the balance of affective to instrumental ties obligating his clients to him—can tell us something about its stability under different conditions. When a patron increases his material resource base, it is his instrumental following that will tend to grow rapidly, and when he is in decline, that same following will shrink rapidly, as clients look for a more promising leader. The degree of dependence on material incentives within a following is, in principle, a quality one could measure by establishing how much more than their present material rewards a rival patron would have to offer to detach a given number of another's clients.

The affective-instrumental distinction just made leads to a similar, but not identical, distinction between the *core* and *periphery* of a man's following. These categories actually are distributed along a continuum; at the *periphery* of a man's following are those clients who are relatively easy to detach while at the *core* are followers who are more firmly bound to him. The periphery is composed of clients bound largely by instrumental rewards, while the core is composed of clients linked by strong affective ties, *as well as* clients who are attracted to a patron by such strong instrumental ties that they seem unbreakable.[30] This amounts, in effect, to a distinction between a man's virtually irreducible following and his more or less fluctuating, "fair-weather" following. Patrons can then be differentiated by the size of their core-following relative to their peripheral-following. A landlord or a businessman will generally have a sizable core group composed both of his friends, kin, etc., and of his tenants or employees. This nucleus is his initial following; his clientele may grow larger, but it is unlikely to contract further than this durable core. A politician or bureaucrat, on the other hand, unless he is privately wealthy, is likely to have a comparatively smaller core group composed mostly of those with whom he has strong affective ties and, hence, a relatively large proportion of "fair-weather" clients. The blows of fortune such a politician or administrator suffers are more likely to be instantly and fully reflected in a reduction of the size of his clientele, which is largely a calculating one. Politicians, and bureaucrats, because they have smaller core followings and because they can, through their office, often tap vast resources, are apt to have meteoric qualities as patrons; the landholder, by contrast, is likely to cast a steadier, if dimmer, light.

Balance of Voluntarism and Coercion. There are obvious and important differences in the degree of coercion involved in a patron-client bond. At one end are the clients with virtually no choice but to follow the patron who directly controls their means of subsistence. Here one might place a tenant whose landlord provides his physical security, his land, his implements and seed, in a society where land is scarce and insecurity rife. Nearer the middle of this continuum would perhaps be the bonds between independent smallholders who depend on a landlord for the milling and marketing of their crops, for small loans, and for assistance with the police and administration. Such bonds are still based on inequality, but the client, because he has some bargaining power, is not simply putty in his patron's hands. Finally, let us assume that an electoral system has given clients a new resource and has spurred competition among patrons for followings that can swing the election to them. In this case the inequality in bargaining power is further reduced, and the client emerges as more nearly an independent political actor whose demands will receive a full hearing from his patron.

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[29] There is no contradiction, I believe, in holding that a patron-client link originates in a power relationship and also holding that genuine affective ties reinforce that link. Affective ties often help legitimate a relationship that is rooted in inequality. For an argument that, in contrast, begins with the assumption that some cultures engender a psychological need for dependence, see Dominique O. Mannoni, *Prospero and Caliban: The Psychology of Colonization* (New York: Praeger, 1964).

[30] F. G. Bailey uses the terms "core" and "support" in much the same fashion: see his "Parapolitical Systems," in Swartz, ed., *Local-Level Politics*, pp. 281–294.

In general, the oppression of the client is greater when the patron's services are vital, when he exercises a monopoly over their distribution, and when he has little need for clients himself. The freedom of the client is enhanced most when there are many patrons whose services are not vital and who compete with one another to assemble a large clientele—say for electoral purposes.

The greater the coercive power of the patron vis-à-vis his client, the fewer rewards he must supply to retain him. A patron in a strong position is more likely to employ *sanctions*—threats to punish the client or to withdraw benefits he currently enjoys—whereas a relatively weaker patron is more likely to offer inducements—promises to reward a client with benefits he does not now enjoy.[31] In each instance, superior control over resources is used to gain the compliance of followers, but the use of sanctions indicates a higher order of power than the use of inducements.

Assessment of the coercive balance and of the ratio of sanctions to inducements can be made not only for a dyad but also for a patron-client cluster or pyramid. The cluster of a local baron with a private army may be held intact by a mix of deference and sanctions, while a campaigning politician may build a cluster simply with favors if he has no coercive power or traditional legitimacy. Each cluster or pyramid has its special vulnerability. The coercive cluster will be jeopardized by a breach of the patron's local power monopoly, and a cluster based on inducements will be in danger if its leader's income or access to public funds is cut off.

Durability Over Time. Patron-client dyads may be rather ephemeral, or they may persist for long periods.[32] In a traditional setting they are likely to last until one of the partners has died. Knowing how durable such ties are can also tell us something about the structure of competition over time. Where dyads are persistent they tend to produce persistent factional structures with some continuity in personnel over time, at least stable clusters or pyramids that may recombine in a variety of ways but are constructed from the same components. Where dyads are fragile, personal alignments may undergo an almost total reordering within a decade.

Since patron-client clusters are based ultimately on power relations, they will endure best in a stable setting that preserves existing power positions. A particular patron will thus retain his clients as long as he continues to dominate the supply of services they need. A patron is also likely to keep his followers if the scope of reciprocity that binds them is greater. That is, the more of the client's vital needs a patron can meet (i.e., if he can supply not only land and security but also influence with the administration, help in arranging mortgages or schooling, and so forth), the greater the tendency for the tie to be invoked frequently and to endure over long periods. Compared with patrons who can provide only legal services, only financial help, or only educational advantages, the *multiplex* bond between patron and client is a solid linkage that serves many needs; since it is more of a whole-person tie, it will be called into action often.

Homogeneity of Following. A patron may have a heterogeneous set of followers drawn from all walks of life, or he may have a following composed, say, of only his poor sharecroppers or only clerical subordinates in his office.[33] The proportion of a man's supporters who share social characteristics and the salience of those social characteristics to them constitute a measure of how homogeneous a following is. Since a patron, by definition, occupies a higher social station than his clients, the greater the homogeneity in a following, the greater the latent shared interests among followers that might threaten the relationship. When a landed patron whose clients are his tenants, for example, sells off what had been common pasture land, all his tenants are equally affected. Their shared situation and the common experiences it provides create a potential for horizontal ties, whereas a heterogeneous clientele lacks this potential.

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[31] Here I follow the distinctions made in Blau, *Exchange and Power in Social Life*, pp. 115–118. Other power theorists have made the same distinction.

[32] Both J. A. Barnes, "Networks and Political Process," in Swartz, pp. 107–130, and Powell, *Peasant Society* . . . , p. 413, discuss this variable.

[33] This variable thus relates not to a dyad but to the following in a cluster or pyramid.

Field Variables. Occasionally, we will want to describe and contrast configurations of patron-client clusters within a political arena rather than dealing with a single cluster or dyad. Four particularly useful distinctions in this respect are a) the degree of monopoly over local resources by a single patron, b) the degree of monopoly over links to other structures by a single patron, c) the *density* of patron-client linkages in the population,[34] and d) the extent of differentiation between different pyramids and clusters. The first two variables are self-explanatory and measure the degree of dominance exercised by a patron over local and supralocal resources. "Density" refers to the proportion of a given population that is a part of the patron-client network. In some situations, for example, a large part of the lower classes may not actually have any vertical links of clientage to a patron. To gauge accurately the explanatory power of patron-client politics in a political field requires that we know for how much of the population such ties are effective. Finally, the degree of differentiation among clusters is a means of discerning whether one cluster looks pretty much like the next one or whether many clusters are socioeconomically distinct. In the classical feudal situation, the pyramidal structure of one lord's small domain was similar to that of his neighbor—the social structure of the landscape resembled a repetitive wallpaper pattern—and competition was thus between almost identical units. In other circumstances, pyramids may be differentiated by predominant occupation, by institutional affiliation, and so forth, so that the seeds of a distinctive and perhaps durable *interest* have been sown.

III. Survival and Development of Patron-Client Ties in Southeast Asia

A. Conditions for Survival

As units of political structure, patron-client clusters not only typify both local and national politics in Southeast Asia, they are also as characteristic of the area's contemporary politics as of its traditional politics. In one sense, the "style" of the patron-client link, regardless of its context, is distinctively traditional. It is particularistic where (following Parsons) modern links are universal; it is diffuse and informal where modern ties are specific or contractual; and it produces vertically-integrated groups with shifting interests rather than horizontally-integrated groups with durable interests. Despite their traditional style, however, patron-client clusters both serve as mechanisms for bringing together individuals who are not kinsmen and as the building-blocks for elaborate networks of vertical integration. They cannot, therefore, be merely dismissed as vestigial remains of archaic structures but must be analyzed as a type of social bond that may be dominant in some contexts and marginal in others.

In my view, most of traditional and contemporary Southeast Asia has met three necessary conditions for the continued vitality of patron-client structures: (1) *the persistence of marked inequalities in the control of wealth, status, and power which have been accepted (until recently) as more or less legitimate;* (2) *the relative absence of firm, impersonal guarantees of physical security, status and position, or wealth,* and (3) *the inability of the kinship unit to serve as an effective vehicle for personal security or advancement.*

The first condition is more or less self-evident. A client affiliates with a patron by virtue of the patron's superior access to important goods and services. This inequality is an expression of a stratification system which serves as the basis for vertical exchange. Classically in Southeast Asia, the patron has depended more on the local organization of force and access to office as the sinews of his leadership than upon hereditary status or land ownership. Inequalities were thus marked, but elite circulation tended to be comparatively high. With the penetration of colonial government and commercialization of the economy, land ownership made its appearance (especially in the Philippines and Vietnam) as a major basis of patronage. At the same time access to colonial office replaced to some extent victory in the previously more fluid local power contests as the criterion for local patronage. Although land ownership and bureaucratic office have remained two significant bases of patronship in postcolonial Southeast Asia, they have been joined—and sometimes eclipsed as patronage resources—by office in political parties or military rank.

If inequities in access to vital goods were alone sufficient to promote the expansion of patron-client ties, such structures would predominate almost everywhere. A second, and more significant, condition of patron-client politics is the absence of institutional guarantees for an individual's security, status, or wealth. Where consensus has produced an institutionalized means of indirect exchange—one that is legally based, uniformly enforced, and effective—impersonal contractual arrangements tend to usurp the place of personal reciprocity. A patron-client dyad, by contrast, is a *personal* security mechanism and is resorted to most often when personal security is frequently in jeopardy and when impersonal social controls are unreliable. In this context, direct personal ties based on reciprocity substitute for law, shared values, and strong institutions. As Eric Wolf has noted, "The clearest gain from such a (patron-client) relation . . . is in situations where public law cannot guarantee adequate protection against breaches of non-kin contracts."[35]

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[34] My use of the term is adapted from Barnes, p. 117.

It is important to recognize the unenviable situation of the typical client in less developed nations. Since he lives in an environment of scarcity, competition for wealth and power is seen as a zero-sum contest in which his losses are another's gain and vice-versa.[36] His very survival is constantly threatened by the caprice of nature and by social forces beyond his control. In such an environment, where subsistence needs are paramount and physical security uncertain, a modicum of protection and insurance can often be gained only by depending on a superior who undertakes personally to provide for his own clients. Operating with such a slim margin, the client prefers to minimize his losses—at the cost of his independence—rather than to maximize his gains by taking risks he cannot afford. When one's physical security and means of livelihood are problematic, and when recourse to law is unavailable or unreliable, the social value of a personal defender is maximized.

The growth of strong, institutional orders that reduce the need for personal alliances was a rare occurrence—the Roman and Chinese imperial orders being the most notable exceptions—until the 19th and 20th centuries, when modern nation-states developed the technical means to impose their will throughout their territory. Before that, however, the existence of a fair degree of local autonomy was inevitable, given the limited power available to most traditional kingdoms. The greater that autonomy, or what might be called the *localization* of power, the more decisive patron-client linkages were likely to be. In settings as diverse as much of Latin America, feudal Europe, and precolonial Southeast Asia, the localization of power was pervasive and gave rise to networks of patron-client bonds. From time to time in Southeast Asia a centralizing kingdom managed to extend its power over wide areas, but seldom for very long or with a uniform system of authority. A typical Southeast Asian kingdom's authority weakened steadily with increasing distance from the capital city. Beyond the immediate environs of the court, the ruler was normally reduced to choosing which of a number of competing petty chiefs with local power bases he would prefer to back.[37] Such chiefs retained their own personal following; their relationship to the ruler was one of bargaining as well as deference; and they might back a rival claimant to the throne or simply defy demands of the court when they were dissatisfied with their patron's behavior. Thus, the political structure of traditional Southeast Asia favored the growth of patron-client links, inasmuch as it was necessary for peasants to accommodate themselves to the continuing reality of autonomous personal authority at almost all levels.

The localization of power is in many senses as striking a characteristic of contemporary as of traditional Southeast Asia. As Huntington aptly expressed it, "The most important political distinction among countries concerns not their form of government but their degree of government."[38] Many of the outlying areas of Southeast Asian nations, particularly the upland regions of slash-and-burn agriculturalists, are only intermittently subject to central government control and continue to operate with much autonomy. By far the most important manifestation of the localization of power, however, has occurred within the very bureaucratic and political institutions that are associated with a central state. The modern institutional framework is a relatively recent import in Southeast Asia; it finds minimal support from indigenous social values and receives only sporadic legal enforcement. With the exception of North Vietnam and Singapore, where a portion of the intelligentsia with modernizing ideologies and popular backing have taken power, these new institutions do not command wide loyalty and must therefore fight for survival in a hostile environment. The net effect of this fragile institutional order is to promote the growth of personal spheres of influence within ministries, administrative agencies, and parties. Sometimes the vertical links are strong (e.g., Thailand) and sometimes a high degree of decentralization or "sub-infeudation" occurs (as in parliamentary Burma from 1955 to 1958). In either case, what replaces the institution are elaborate networks of personal patron-client ties that carry on more or less traditional factional struggles rather than operate as agents of a hierarchial organization. Patron-client politics are thus as much a characteristic of faction-ridden central institutions as of the geographical periphery in these nations.

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[35] Wolf, "Kinship, Friendship, and Patron-Client Relations . . . ," p. 10.

[36] In this connection, see my *Political Ideology in Malaysia* (New Haven: Yale University Press, 1968), chapter 6; and for zero-sum conceptions among peasants, see George M. Foster, "Peasant Society and the Image of Limited Good," *American Anthropologist*, 65 (April, 1965), 293–315.

[37] See, for example, Edmund R. Leach, "The Frontiers of Burma," *Comparative Studies in Society and History*, 3 (October, 1960), 49–68.

[38] Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968), p. 3.

The third condition under which patron-client bonds remain prominent relates directly to the capacity of such ties to foster cooperation among nonkin. As a mechanism for protection or for advancement, patron-client dyads will flourish when kinship bonds alone become inadequate for these purposes.

Although kinship bonds are seldom completely adequate as structures of protection and advancement even in the simplest societies, they may perform these functions well enough to minimize the need for nonkin structures. Such is the case among small isolated bands of hunters and gatherers, among self-sufficient, corporate lineages and within corporate villages.[39] None of these conditions, however, is particularly applicable to Southeast Asian societies. The highland areas are inhabited by poorly integrated minorities but only rarely are these minorities so isolated as to lack economic and political ties with the larger society. Corporate lineages, outside traditional Vietnam, are uncommon in low-land Southeast Asis where bilateral kinship systems lead to overlapping kindreds rather than mutually exclusive lineages. Finally, corporate village structures (except in Java and perhaps Vietnam's Red River delta) are not typical of Southeast Asia. The scope for nonkin ties in general, and patron-client links in particular, has thus been quite wide throughout the region.

Even when government did not impinge much on their activities, villagers in traditional Southeast Asia still had need of extrakin and extravillage contacts. They needed to secure marriage partners, to assure themselves protection and contacts for the limited but vital trade carried on between villages, and finally to establish an outside alliance in case a village quarrel forced them to seek land and employment elsewhere.[40] If vertical dyadic ties were of some value in the traditional context, they assumed a more decisive role in the colonial and postcolonial periods. First, the commercialization of the economy and the growth of markets enhanced the value of cooperative arrangements among nonkin. Both corporate kin groups and corporate village structures had depended on a certain level of economic autarchy for their vitality—an autarchy which colonial economic policy quickly eroded. These corporate structures (where they existed) tended "to lose their monopoly over resources and personnel in situations where land and labor became free commodities."[41] As the communal land controlled by the village dwindled, as outsiders came increasingly to own land in the village, and as villagers increasingly worked for nonkin, the value of patron-client links increased for all concerned.

In traditional Southeast Asia, as in feudal Europe, then, the inability of kindreds to provide adequate protection and security fostered the growth of patron-client structures. The limited effectiveness of kindreds as units of cooperation and security was further reduced by the new structures and uncertainties of the colonial economy. Within this new economy, the goals of wealth, protection, power, and status could not be realized without outside links to nonkin (and often nonvillagers), and the establishment of these links, for the most part, followed the patron-client model.

The relative decline in the protective capacity of kindreds (which, given the absence of strong, predictable new institutions would widen the scope of patron-client ties) accelerated the *political* transformation of the colonial and postcolonial period as well. Both administration and electoral politics created new political units that did not generally coincide with the kindred or with the traditional village. As

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[39] Corporate villages are included here since they generally stress shared kinship links to a common ancestor. Part of the corporate character of the Javanese village was perhaps further reinforced as a consequence of the collective exactions required by Dutch colonial policy. "Sanctioned reciprocity" is probably a better term for village structures in Java and Tokin than "corporate."

[40] Richard Downs, "A Kelantanese Village of Malaya," in Julian H. Steward, ed., *Contemporary Change in Traditional Societies*, Vol. II: *Asian Rural Societies* (Champaign-Urbana, Illinois: University of Illinois Press, 1967), p. 147.

[41] Wolf, "Kinship, Friendship, and Patron-Client Relations . . . ," p. 5. For a brilliant account of the same process in England, see Karl Polanyi, *The Great Transformation* (Boston: Beacon Press, 1957).

Gallin has shown for electoral politics in Taiwan, the political vitality of the corporate lineage is sapped by changes in the governing unit which no longer permit a single lineage to dominate. The lineage thus loses much of the material basis of its previous solidarity, and new dyadic ties become the means by which winning coalitions are built in the new unit.[42] Political consolidation, like economic consolidation, beyond minimal kin and village units can thus enlarge the potential role of such nonkin structures as patron-client clusters.

Considering all three criteria, Southeast Asian states, like most traditional nations, satisfy most conditions for the survival of patron-client structures as a common means of cooperation. First, the disparities in power and status that form the basis of this kind of exchange have, if anything, become more marked since the colonial period. Second, nonkin structures of cooperation have always been important in the complex societies of Southeast Asia and have become more significant because of the new economic and political dependencies introduced by colonialism and nationhood. Finally, with the possible exceptions of Singapore and North Vietnam, the nations of Southeast Asia have not developed strong modern institutions which would begin to undermine purely personal alliance systems with impersonal guarantees and loyalties.

At this point in the argument, it is essential to show how patron-client structures, as one form of vertical cleavage, coexist with communalism, another form of vertical cleavage, in Southeast Asia. If loyalty to an ethnic or religious group is particularly strong it will mean that the only possible partners in most patron-client dyads will be other members of the same community. Since the community is a categorical group which excludes some possible dyadic partnerships, it represents a different form of cleavage from patron-client links. Vertical dyadic bonds can, nonetheless, coexist with communal cleavage in at least three ways: 1) as intercommunal patron-client ties above corporate communities; 2) as intercommunal patron-client ties above factionalized communities, and, 3) as intracommunal patron-client structures. First, when communal groups do deal corporately with the outside—as quite a few small, highland tribes do in Southeast Asia—we may get patron-client ties that join their leaders, as clients, to regional or national leaders. If two distinct corporate communities were linked through their leaders who were clients of an outside leader, the structure could look like that in Figure A.

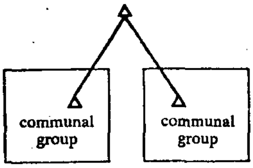

Figure A

More often in Southeast Asia, however, the second situation prevails in which a number of *patrons with separate followings within the same communal group* compete for the most advantageous links to the outside. A simple representation of this pattern is presented in Figure B, in which two patrons are linked as clients to an outside patron and thereby have established a working alliance against a third patron in the same communal group who is linked to a different outside leader. Here, the communal group is rent by factionalism and has multiple ties to the outside world.[43] The vertical links outside the communal group, however, are likely to be somewhat weaker or more tentative than links within the community. This is so because all competing subordinate patrons and their clientele fall within a communal unit which shares a potentially strong interest; if the communal group as a whole were threatened, the shared parochial links would serve as the basis for a unity that might supersede any exterior patron-client links. The situation described in Figure B is only likely to arise, then, if there are no salient

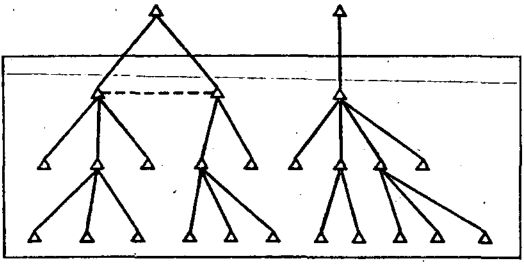

Figure B

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[42] Bernard Gallin, "Political Factionalism and Its Impact on Chinese Village School Organization in Taiwan," in Swartz, ed., pp. 377–400.

[43] A combination of situations one and two would occur when the tacit rules within a communal group allowed patron-client conflict but forbade the losing or weaker patrons within the communal group from maintaining ties to outside leaders.

collective threats to the communal group as a whole.

The mixture of communalism and patron-client structures portrayed in Figures A and B focuses on the extracommunal patron-client links that achieve a measure, however weak, of intercommunal integration. A third mixture of communal and dyadic association focuses instead on intracommunal politics alone. This would be represented by just the boxed portion of Figure B, which indicates that, even if communal conflict is widespread, it may well be that the *intra*communal politics of each contending group is best described by the patron-client model.

The salience of communal feeling, especially in Malaysia, Burma, and Laos, but also in Indonesia and Vietnam makes such mixtures of communalism and patron-client politics common. Except at the apex of the political structure where a leader may have leaders of smaller communal groups as his clients, most patrons have followings that are almost exclusively drawn from their own community. Intercommunal integration tends to take place near the apex of the political structure with the base of each communal pyramid remaining largely separate. The links that represent this integration tend, moreover, to be fragile and to disintegrate in the face of a communitywide threat. Both communalism and patron-client links share the political stage, but patron-client structures are most prominent in periods of peace and stability. In addition, the process of politics *within* each communal group—in effect holding communal affiliation constant—is usually best analyzed along patron-client lines. In nations such as Thailand, the Philippines, and Cambodia, which are comparatively homogeneous culturally, there are few communal barriers to the proliferation of patron-client linkages. Thus, the patron-client model can be applied to those nations in its "pure" form since communal affiliation is not important in creating discontinuous patron-client networks.

B. The Transformation of Traditional Patron-Client Ties

1. The General Trend. The typical patron in traditional Southeast Asia was a petty local leader. Unlike the representative of a corporate kin group or a corporate village structure (rare outside Vietnam and Java, respectively), the local patron owed his local leadership to his personal skills, his wealth, and occasionally to his connections with regional leaders—all of which enhanced his capacity to build a personal following. The fortunes of such petty leaders waxed or waned depending on the continuing availability of resources and spoils which served to knit together a following. Perhaps the most striking feature of local patron leadership in Southeast Asia was its fluidity and instability, which contributed to a relatively high rate of local elite circulation. In contrast to India, where hereditary office-holding and landholding provided somewhat greater continuity, the typical local leader in Southeast Asia had put together many of the necessary resources of wealth, force, connections, and status on his own and could probably only promise his son a slight advantage in the next round. Two important reasons for this oscillation in local power are a) the weakness of the central state, which lacked either the force or durability to sustain and guarantee the continuation of local power elites, and b) the relative ease with which clients in a slash-and-burn economy could, if dissatisfied simply move to another area, thus undermining their ex-patron's basis of power.[44]

Patron-client systems have survived—even flourished—in both colonial and postindependence Southeast Asia. There have been important changes, however. New resources for patronage, such as party connections, development programs, nationalized enterprises, and bureaucratic power have been created. Patron-client structures are now more closely linked to the national level with jobs, cash, and petty favors flowing down the network, and votes or support flowing upward. In the midst of this change, old style patrons still thrive. Highland leaders, for example, still operate in a personal capacity as patron/brokers for their people with lowland leaders. Landowners in the Philippines and elsewhere have used their traditional control of land and the tenants who farm it to win positions of local or regional party leadership. Whatever the particular form they take, patron-client networks still function as the main basis of alliance systems among nonkin throughout Southeast Asia.

The nature of patron-client bonds within Southeast Asia has varied sharply from one period to the next and from one location to another. Different resources have risen or plummeted in value as a basis of patronage depending upon the nature of the political system:

(See table on next page.)

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[44] See, for example, Edmund R. Leach, *The Political Systems of Highland Burma* (Cambridge: Harvard University Press, 1954), and J. M. Gullick, *Indigenous Political Systems of Western Malaya,* London School of Economics Monographs on Social Anthropology #17 (London: University of London/Athlone Press, 1958).

Secular Trends in the Nature of Patron-Client Ties in Southeast Asia

Quality	*Traditional*	*Contemporary*
1. Duration of bond	more persistent	less persistent
2. Scope of exchange	multiplex	[increasingly] simplex
3. Resource base	local, personal	external links, office-based
4. Affective/instrumental balance	higher ratio of affective to instrumental ties	lower ratio of affective to instrumental ties
5. Local resource control	more local monopoly	less local monopoly
6. Differentiation between clusters	less differentiation	more differentiation
7. Density of coverage	greater density	less density

The capacity to mobilize an armed following was particularly valuable in the precolonial era; access to colonial office was a surer basis of patronage than armed force in the colonial period; and the ability to win electoral contests often became the central resource with the advent of independence. Not only have resource bases proved mercurial over time, but the nature of patron-client ties in the indirectly ruled highland areas has remained substantially different from lowland patterns. Amidst this variety and change, it is nevertheless possible to discern a number of secular trends in the character of patron-client bonds. Such trends are far more pronounced in some areas than others, but they do represent directions of change that are important for our analysis.

(1) In comparison with more bureaucratic empires, patron-client bonds in precolonial Southeast Asia were not, as I have pointed out, markedly persistent. With the quickening of social change brought about by the commercialization of the economy and the penetration of the colonial state into local affairs, however, a patron's resource base became even more vulnerable to the actions of outside forces over which he had little or no control. It was an ingenious patron indeed who could survive the creation of the colonial state, the export boom, the depression of the 1930s, the Japanese occupation, and independence with his resources and clientele intact. The major exception to this trend was the colonial period in indirectly ruled areas where colonial military and financial backing of traditional rulers, if anything, brought a stability—or stagnation—to political systems that had been more chaotic. Elsewhere, patron-client links tended to become more fragile and less persistent.

(2) With the differentiation of the economy and its effects on the social structure, the scope of exchange between patron and client tended to narrow somewhat. Where traditional patrons could generally serve as all purpose protectors, the newer patron's effectiveness tended to be more specialized in areas such as political influence, modern sector employment, or administrative influence. Although patron-client ties remained flexible and personal, the more limited capacities of the patron tended to make relationships less comprehensive and hence less stable.[45]

(3) The traditional patron for the most part operated with personally controlled local resources. One effect of the colonial period—and independence as well—was to increase radically the importance of external resources for local patronage. A following based on purely local office or landholding was seldom sufficient to sustain a patron in a new environment where schools, agricultural services, regional banks, and public employment represented competing sources of patronage. The growing role of outside resources, in most cases, thus led to competition among patrons, each of whom recruited followings with the particular resources at his command.[46] In addition, since those who controlled the new resources were generally office-holders subject to transfers or political changes at the center, the new patrons were less secure than older patrons and probably more inclined to maximize their gains over the short run.

(4) Because the new patron-client ties were weaker and less comprehensive, and because the new patrons were often from outside the local community, the instrumental nature of the exchange became more prominent. A relationship that had always involved some calculations of advantage lost some of its traditional legitimacy and grew more profane. Patron-client exchanges became more monetized, calculations more explicit, and concern centered

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[45] Again, indirectly ruled areas were often exceptions in that local rulers tended to take on new powers under the colonial regime and thus became more comprehensive patrons than in the past.

[46] For Malaysia, M. G. Swift, *Malay Peasant Society in Jelebu* (London: University of London, 1965), pp. 158–60 captures this shift in local power. A general treatment of such changes is contained in Ralph W. Nicholas, "Factions: A Comparative Analysis," in M. Banton, general ed., *Political Systems and the Distribution of Power*, Association of Applied Social Anthropology Monograph #2 (London: Tavistock Publications, 1965), pp. 21–61.

more on the rate of return from the relationship rather than on its durability. This trend meant that newer patron-client clusters were likely to have a comparatively large "fair-weather" periphery, a comparatively small core-following, and a less "constant" patron as well.

(5) The breakdown of local patron monopolies follows logically from most of the changes we have already discussed. Where one local landowner or traditional leader had once dominated he now faced competitors who might be local administrators of state welfare in loan programs, teachers in new secular schools, a local trader or businessman, or the resident manager of a foreign-owned plantation. Factional strife which reflects this competition was most common in villages where socioeconomic change and government penetration had been far-reaching, and less common in more traditional areas.[47]

(6) As differentiation occurred within the local societies, they gave rise to patron-client clusters that were distinct. A bureaucrat might have a following primarily within his agency, a businessman among his laborers, and a landowner among his tenants. This process of differentiation among clusters provided the potential basis for durable group interests inasmuch as many clusters now had an institutional distinctiveness.

(7) While the changes we have examined may have assisted the vertical integration of patron-client pyramids, they tended to reduce the universality of coverage. That is, more and more people in the new market towns and cities, on plantations, and on small plots they rented from absentee landlords were no longer attached—or were very weakly attached—to patrons. These new elements of the population varied greatly in their interests and their levels of organization, but, in any event, they fell outside the older patron-client network.

While some long run trends in patron-client ties seem clear, it is difficult to say anything about the balance between voluntarism and coercion over time. On the one hand, changes in the economy have made clients less autonomous and more dependent on patrons for protection against a fall in world prices, for cash advances before the harvest, and so forth. Also contributing to a decline in the client's bargaining position is the imported legal system of property guarantees which allow a wealthy man, if he so chooses, to resist pressures for redistribution that operated in a traditional setting. On the other hand, the breakdown of local patronly monopolies and the exchange resources that electoral systems often place in the hands of clients work in the opposite direction. Given these contradictory tendencies, one can draw the tentative conclusion that patron coerciveness has declined only where extra-local resources *and* competitive elections are common and has elsewhere either increased or remained the same.

In general, patron-client ties have tended to become more instrumental, less comprehensive, and hence less resilient. They still represent diffuse personal bonds of affection when compared to the impersonal, contractual ties of the marketplace, but the direction of change is eroding their more traditional characteristics. Even this supple traditional protective mechanism has had to pay a certain price to survive in the midst of a nation-state with a commercialized economy. The durability and legitimacy of the patron-client tie was best served when all of a client's dependencies were focused on a single patron. But, as Godfrey and Monica Wilson have shown, this situation is less and less likely since the process of modernization tends to create multiple dependencies—each of less intensity—rather than concentrating dependence on one person.[48] The slowly weakening comprehensiveness of the link is, ultimately, what undermines its sanctity and legitimacy for the client.

2. The Dynamics of the Transformation. The engine behind the shift in patron-client ties was largely provided by the penetration of the local arena by an intrusive national economy and national political system. This penetration wrought two major changes that transformed patron-client links: a) during the colonial period, especially, it impaired the effectiveness of local redistributive pressures and b) particularly after independence it "nationalized" access to patronly resources, thus creating new bases of patronage and devaluating old ones.

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[47] In his study of politics in an Indonesian town, Clifford Geertz has shown that the more traditional hamlets were more likely to be united under a particular leader than were hamlets which had changed more; *The Social History of an Indonesian Town* (Cambridge: M.I.T. Press, 1965), Chapter 6. This finding is corroborated by Feith's study of the 1955 Indonesian elections; Herbert Feith, *The Indonesian Elections of 1955*, Interim Report Series, Modern Indonesia Project (Ithaca: Cornell University, 1961), pp. 28–30. A comparative study of two Burmese villages also supports this conclusion: cf. Manning Nash, *The Golden Road to Modernity* (New York: Wiley, 1965). In this context, directly ruled lowland areas tended to develop factional competition among different patrons, while less directly ruled areas (especially highland areas) more frequently retained some unity behind a single patron who remained their broker with the outside world.

[48] G. and M. Wilson, *The Analysis of Social Change*, pp. 28, 40.

Traditional peasant societies, operating in an economy of great scarcity in which one family's gain is another's loss, have generally developed a variety of social control mechanisms that guarantee a measure of security to each family and temper the centrifugal forces generated by the struggle for subsistence.[49] These mechanisms commonly involve forcing anyone who has accumulated considerable wealth to redistribute a portion of it. A wealthy man is pressed to assume expensive ceremonial offices, to make large religious contributions, to give loans and donations, and so forth. He trades his wealth for prestige, and, by so providing for at least the minimum well-being of others, he becomes a legitimate patron with a personal entourage of those obligated to him.

The central fact about these redistributive mechanisms, however, is that they operate by virtue of a local power situation. That is, the wealthy man in a peasant village can seldom rely on outside force or law to protect him; instead, his wealth and position are ultimately validated by the legitimacy he acquires in the local community. Unless a wealthy individual can persuade most of the community that his wealth is no threat to them or can win enough personal allies to sustain his position, he is in danger. Colonialism, however, broke the relative autonomy of the local arena and hence weakened many of the community's redistributive pressures. Supported in effect by the power of the colonial regime to enforce its notion of law, the patron could increasingly ignore local levelling pressures. If he lost much of the social approval he previously enjoyed, he had gained an outside ally with the power to guarantee his local position. The colonial power situation thus offered the *older* patron new leverage in the local arena—leverage which was further strengthened by the growing complexity of colonial society. As Blau has explained,

> Social approval has a less pervasive significance as a restraining force in complex societies than in simpler ones, because the multiplicity of groups and the possible mobility between them in complex societies allows deviants of nearly all sorts to escape from the impact of community disapproval by finding a sub-group of likeminded persons....[50]

Absentee landlords, the new urban wealthy, and minority communities (who were relatively impervious to local social approval so long as they had colonial backing) were *new* elements in colonial society which could escape patronly obligations. The colonial system thus tended to allow existing patrons greater latitude for exploitation while producing a class of wealthy nonpatrons.

If the intrusion of external power could strengthen the hand of an existing patron, it could also create a resource base for the rise of new patrons. The activities of the colonial regime included the hiring, firing, and promotion of public employees, the dispensing of contracts, and the granting of licenses and permits, all of which could be used to create a personal following. With independence, not only did local leaders take over responsibility for all these decisions, but the scope of government activity and regulation was generally expanded into new areas such as community development. The survival or demise of a local patron often depended, as Geertz has shown, on how successful he was in tapping these new bases of power.[51]

Except for the rare local patrons—especially in indirectly ruled areas—who were able to monopolize these external resources, the new situation produced more competition and mobility among patrons. Many potential clients quickly discovered that their needs were best served by a patron who had access to the institutions which controlled the use of these external resources. In any local context this shift could be measured by the rise of new patrons who were wholly or partly based in these new structures. Stuying the incorporation of Sardinia into the Italian nation-state in the 20th century, Alex Weingrod has documented the growing importance of such externally based patrons.[52] The proportion of outsiders asked to be godparents, for example, increased dramatically from 1920 to 1960, and patrons with links to the ruling party and the state bureaucracy had increased their followings at the expense of traditional landholders. A similar process has occurred in Southeast Asia as the integration of villages into a national economy and political system tended to produce a number of more specialized local patrons who often became factional leaders.

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[49] For a description of such mechanisms see Clifford Geertz, *Agricultural Involution* (Berkeley: University of California Press, 1963); George M. Foster, "Peasant Society and the Image of Limited Good," *American Anthropologist* 67, 2 (April, 1965), pp. 293–315; Swift, *Malay Peasant Society . . .* , and Mary B. Hollnsteiner, "Social Control and Filipino Personality," *Symposium on the Filipino Personality* (Malcati: Psychological Association of the Philippines, 1965), p. 24.

[50] Blau, *Exchange and Power in Social Life*, p. 114.

[51] Geertz shows how local leaders often managed to become agents of the local sugar mills—buying crops, renting land, and recruiting labor and thereby enlarging their power in the community. *The Social History . . .* , p. 57.

[52] Alex Weingrod, "Patrons, Patronage, and Political Parties," pp. 388, 397.

Most of the transformations in patron-client bonds that we have been discussing apply with greatest force to the directly ruled, lowland areas of Southeast Asia where the colonial impact was both swift and far-reaching, and where colonial officials more thoroughly replaced indigenous leaders. In the indirectly ruled areas—such as highland Burma, the Unfederated States of Malaya, most of Indonesia's Outer Islands, Cambodia, Laos (and perhaps Thailand belongs here as a limiting case of indirect colonial influence)—these generalizations must be qualified. To ease the financial and administrative burden of colonial rule in these areas, the colonizers generally kept local rulers in place and used them as agents. Since these were by and large peripheral areas of marginal commercial interest, the pace of economic change tended to be slower as well.

The effects of this policy on patrons, in contrast to the directly ruled regions, were twofold. First, local patron/leaders tended to be strengthened by colonial backing and the new powers given to them. What had probably been a fairly unstable and minimal chieftaincy now became a local regime stabilized and extended by the colonial power. Secondly, the sanction of colonial authority permitted many such leaders to broaden the resource base of their authority.[53] It is true of course that a local patron's new source of strength entailed some threat to his legitimacy, but since the colonial regime demanded little beyond the maintenance of law and order in those areas, it was seldom crippling. On the other hand, the annointed patron now had the means to eclipse his rival patrons. He not only had his traditional authority and the discretionary administrative powers given him by the colonial regime, but he could use his power to purchase land, control local trade, and act as the commissioned agent of private firms. Frequently, then, the local ruler gained a new lease on political life as the dominant local figure owing to his wealth, his administrative power, and a measure of traditional legitimacy. Given the slower pace of economic change and state penetration in these regions, the local leader had to contend less with new competitors who flourished amidst such changes. The strength many patrons achieved under indirect rule is nowhere more apparent than in postcolonial elections in which they often could deliver most of their region's vote.

3. Electoral Politics and Patron-Client Ties. Most Southeast Asian states have had functioning electoral systems at one time since their independence. Although only the Philippines retains a parliamentary system, the electoral studies that do exist can tell us much about the effects of party competition on patron-client bonds and, beyond that, highlight some of the unstable features of patron-client democracies.

The dynamics of electoral competition transformed patron-client relations in at least four important ways: (1) it improved the client's bargaining position with a patron by adding to his resources; (2) it promoted the vertical integration of patron-client structures from the hamlet level to the central government; (3) it led to the creation of new patron-client pyramids and the politicization of old ones; and (4) it contributed to the survival of opposition patron-client pyramids at the local level.

First, with popular elections the client gained a new political resource, since the mere giving or withholding of his vote affected the fortunes of aspirants for office. Nor were voters slow to realize that this resource could be turned to good account. Even someone with no other services of value to offer a patron found that the votes of his immediate family were often sufficient to secure the continuous assistance of a local politician. This pattern could be found throughout Southeast Asia in electoral situations but is most striking in the Philippines, where most patron-client ties are centered around landholding and elections. The Filipino politician, as Wurfel points out, does favors *individually* rather than collectively because he wishes to create a personal obligation of clientship.[54] The voter, for his part, asks that his patron/politician favor him because of a personal obligation to reciprocate.

In one sense, popular elections can be seen as a reestablishment of the redistributive mechanisms of the traditional setting. Once again a patron's position becomes somewhat more dependent on the social approval of his community—a social approval that is now backed by the power to defeat him or his candidate at the polls. Unable to depend on outright coercion, and faced with competitors, the electoral patron knows he must (unless his local economic power is decisive) generally offer his clients better terms than his rivals if he hopes to maintain his local power.

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[53] Similar assessments of the effects of indirect rule can be found in M. G. Swift, pp. 148–149; Harry J. Benda and John Bastin, *A History of Modern Southeast Asia* (Englewood Cliffs, N.J.: Prentice-Hall, 1968), pp. 75–122. The best accounts of the pattern, however, come from India. Cf. Bailey, *Politics and Social Change*, Chapter 4, and Paul R. Brass, *Factional Politics in an Indian State* (Los Angeles and Berkeley: University of California Press, 1965), Chapter 4.

[54] David Wurfel, "The Philippines," in *Comparative Studies in Political Finance: A Symposium, Journal of Politics*, 25 (November, 1963), 757–773.

Second, nationwide elections make it necessary for a national party to establish a network of links extending down to the local level. For the most part a party does this by taking advantage of existing patron-client clusters and incorporating them into its structure. The competitive struggle of Indonesian parties to forge such links in Java during the 1950s is apparent in the accounts Feith and Geertz give of electoral campaigns.[55] They agree that effective campaigning in the village took the form of activating and politicizing preexisting personal links rather than mass meetings or policy stands. The campaign was, as Feith says

> . . . a race for a foothold in these villages . . . a foothold involving allegiance of as many as possible of their influential people. Here the first step was to secure the support of those whose authority was accepted by the village prominents. Thus the parties struggled with one another for influence with the bupatis, the wedanas, and the tjamats . . . with the local military commanders and the heads of local offices of Religion, Information, and Mass Education, kijaijis, ulamas, heads of clans, old guerrilla leaders. . . .[56]

As elsewhere in Southeast Asia, a party succeeded best at the polls by securing the adhesion of the important local patrons, who would deliver their clients as a matter of course. Working on voters individually or by class affiliation made little sense when most of the electorate was divided into patron-client clusters. The affiliation of patrons was often gained by making them candidates, by promising them jobs or other patronage, or even by cash payments. It is clear, however, that, in comparison with those parties who had to create new links to village leaders, parties such as the Nahdatul Ulama which could rely on bonds that antedated the election, were in a stronger position. Nash's account of the 1960 election in upper Burma reveals a similar pattern of patron mobilization. When a local patron was approached to join U Nu's faction of the AFPFL on the promise of later patronage, he was able to get thirty-nine others—his relatives and those who owed him money or for whom he had done favors, i.e., his clients—to join as well.[57]

The nature of the new exchange relationship that gives vitality to this patron-client pyramid is similar in most electoral systems. The local patrons and their clients provide votes at election time, hopefully carrying the village, while the party undertakes to help its local adherents (through their patron) with jobs, help in dealing with the bureaucracy, providing public works, and so forth. Since the winning party can generally offer more support to local allies than the opposition can, local patrons are likely to display a "bandwagon effect," switching allegiance to a probable winner. In addition, the party's need for a powerful local base is likely to lead to a certain localization of power. In return for delivering local votes for its list, the party is likely to give its local patron a wide discretion in administrative and development decisions affecting the locality. Thus many local patrons are able to entrench themselves further as dominant figures.

A third consequence of elections for the patron-client structure is to promote the expansion of patron-client ties and the politicization of existing bonds. Knowing that an electoral victory is important, a local patron with a modest following will probably try to obligate more clients to him in order to strengthen his electoral position. Patrons who have previously been politically inactive would "immediately convert their private power such as control over sharecroppers, debtors kinsmen, neighbors, etc., into public political power in the form of votes."[58] Given these tendencies, the patron-client structures in a given community are most evident immediately before an election, especially a hotly contested one, when the contestants attempt to activate any links that might advance their cause.

A final point about the impact of elections on patron-client structures is that they tend to heighten factionalism and unless one cohesive party completely dominates, to promote the survival of local opposition factions. In most traditional settings, patron rivalry was largely limited to the local arena so as not to invite external intervention. An electoral system, by contrast, creates rival national or regional parties which need allies at the local level. A weak faction that might previously have been forced to compose its differences with a dominant faction, can now appeal for external support. Many of these external allies are able to provide their local adherents with patronage, cash, or other favors so as to maintain a local foothold. The net effect of electoral competition is thus to exacerbate many of the latent factional differences among patron-client clusters and occasionally to buttress weak patrons whose position would otherwise have disintegrated.

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[55] Feith, *The Indonesian Elections of 1955;* Geertz, *The Social History of an Indonesian Town.*

[56] Feith, p. 79.

[57] Manning Nash, "Party Building in Upper Burma," *Asian Survey,* 3 (April, 1963), pp. 196–202.

[58] Nicholas, "Factions . . . ," p. 45.

The effects we have attributed to elections can be compared to the situation in Thailand, where elections have only rarely been any more than a device to legitimate self-selected rulers. There the local client's vote is not important enough to materially improve his bargaining position with a patron, and the vertical integration of patron-client clusters had not gone very far beyond the central institutions of the bureaucracy and armed forces. In Thai villages unlike the electoral settings of Java in the 1950s, or the Philippines, many patron-client clusters are of purely local significance and are not highly politicized. Local factional conflict, as a result, is much less striking in Thailand than where competitive elections have helped to subsidize it.

To this point we have focused on the general influence of elections on patron-client structures. Depending on the region and the party in question, however, there has been a noteworthy variation in the connection between party and patron-client structures. The essential distinction is one between a party that has created its own network of patron-client linkages from the center and a party that relies on preexisting patron-client bonds and merely incorporates them into its organization. This corresponds to René Lemarchand's distinction between types of party machines in Africa.

> In situations where micro-level clientelistic structures provide the essential linkages between the party machine and the masses . . . the machine is superimposed upon, and in some ways tributary of, the clientelistic subsystem. A distinction must therefore be drawn between the more orthodox types of machine . . . in which patronage becomes the essential source of cohesion, and what one might call the neo-traditional machine, in which exchange processes between the center and periphery are mediated by, and contingent upon, the operation of traditional forms of clientelism.[59]

As Lemarchand adds, the "orthodox" machine is more dependent on material inducements since its linkages are of more recent origin and hence more instrumental. "Neo-traditional" machines, by contrast, can rely somewhat more heavily on established patterns of deference, though they must bargain with nearly autonomous local patrons. This distinction has validity in Southeast Asia as well, both in accounting for the different styles of politics in the indirectly ruled, more traditional areas as compared to the directly ruled, heavily commercialized areas and also in explaining the structural differences between the more traditionalist as compared to more modern parties. In the indirectly ruled areas, political parties confronted fairly stable constellations of local patron power which would have been difficult to destroy. It was simpler to come to terms with local leaders rather than to try to circumvent them, even though this accommodation tended to divide the party into a coalition of political fiefdoms. In directly ruled areas where competition among patrons and heavily instrumental ties were more common, a party had a greater opportunity to create new linkages but at perhaps greater expense in favors and patronage. Throughout Southeast Asia all parties had to adapt themselves to these differences in social structure in different regions of the country: the PNI of Indonesia operated differently in Central Java than in the Outer Islands; U Nu's AFPFL faction could not win the support of the hill tribes in the same way they won the vote of the lowland Burmese, and the Nationalists in the Philippines campaigned differently in central Luzon than in Mindanao.

There were also systematic differences between the neo-traditionalist and modernist parties within a given area. In Java, for example, the Nahdatul Ulama incorporated more traditional patron-client ties than did the PNI, which could rely more on its access to material rewards and local administration. On the East Coast of Malaya, the Islamic PMIP appealed most to the more traditional peasantry while the ruling UMNO concentrated on the town population and those dependent on federal funds. One would also guess that the Ba Swe faction of the AFPFL had a more instrumental base in towns and the modern sector, while U Nu's faction was more frequently based on existing patron-client links in the countryside. Simply knowing the kind of patron-client bonds a party had created or incorporated could reveal a great deal about the party's cohesiveness, the nature of its local base, and the extent of its reliance on material inducements.

4. The Inflationary Character of "Patron-Client Democracy." The introduction of competitive elections in Southeast Asia increased the pressures on regimes for the downward distribution of tangible benefits. In return for votes flowing up the vertical chain of patron-client structures, each patron depended upon the downward distribution of patronage in the form of administrative favors, land grants, public employment, and so on, in order to keep his own pyramid of followers intact. Elections, by themselves, had shifted the balance of exchange so that it favored the client somewhat more than before. The consequence of this shift in exchange terms was a greater flow of material benefits toward the base of the patron-client network.

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[59] René Lemarchand, "Political Clientelism and Ethnicity: Competing Solidarities in Nation-Building," *American Political Science Review*, 66 (March, 1972).

The strength of the downward distributive pressures generated by electoral procedures in Southeast Asia depended primarily on four variables which are stated below in contrasting terms.

(*See table at bottom of page.*)

Each of the above variables related to the strength of incentives impelling a party to maximize its clientele and the degree to which that clientele will depend on concrete material incentives rather than ties of affection or deference. The first and obvious requirement for distributive pressures is that elections be important in the selection of an elite. Secondly, a shaky regime or party will be in a less advantageous position to resist client demands than a strong one, since an election is an all or nothing affair and uncertainty over the outcome will raise costs; when the race is close, the party in its recruitment efforts knows that the marginal value of the extra dollar, patronage job, or development grant is all the greater. That is one reason why distributive pressures were greater in 1955 in Indonesia when an election fraught with uncertainty would determine which parties would form a coalition, than in Malaysia in 1964 when the question was not whether the Alliance would win but whether or not it would take two thirds of the seats. The importance of social change (item three) is based on the observation that patron-client ties in less traditional areas typically require the patron to deliver more in the way of instrumental, material rewards The maintenance of a loyal patron-client network in a traditional area where deference is strong, will, I assume, cost a party somewhat less in material rewards and favors than will a network of the same size in a built-up area where traditional patron-client bonds have eroded. Finally, a neo-traditionalist party such as UMNO in Malaysia can, in part, rely upon the traditional legitimacy of many of its leaders while a party of "new men" such as the PNI in Indonesia or the AFPFL in Burma has to rely more often on highly instrumental ties. Thus a weak party led by "new men" and relying on votes from among an uprooted population is likely to develop a patchwork patron-client structure that is very expensive to maintain. It is indicative of just how much financial backing such structures require that only a ruling party with access to the public till can generally afford the construction costs.[60]

The distributive pressures experienced by such regimes manifest themselves in familiar ways. Government budgets, and of course deficits, swell quickly with expenditures on education, growing public employment, community development projects, agricultural loans, and so forth. Particularly since votes in Southeast Asia are to be found in the countryside, one would expect that regimes with strong electoral pressures would spend more in the grass-roots rural areas than would regimes without such pressures. Given such pressures, local expenditure is also arranged as much as possible so that benefits can be distributed individually since that arrangement is more appropriate to patron-client exchange patterns. Even with pork-barrel programs a local party leader will claim personal responsibility for the gift and personally help distribute whatever employment or subcontracting it includes. The capacity of the regime to keep its network intact and win

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[60] Adrian C. Mayer seems to have this distinction in mind, in his study of an Indian town comparing the "hard" campaign of the Jan Sangh, which relied on durable social ties and tried to prevent defections, and the local Congress Party, which ran a "soft" campaign of short-term links by promising favors and benefits to intermediaries. See Mayer, "The Significance of Quasi-Groups in the Study of Complex Societies," p. 106.

Distributive Pressures of Elections

A. Strongest when	*B. Weakest when*
1. Elections determine powerholders	1. Elections have marginal significance
2. Regime is weak, unstable regime	2. Regime is strong, stable regime
3. Socioeconomic change is extensive (direct rule, lowland areas)	3. Socioeconomic change is less extensive (indirect rule, highland areas)
4. Party is modernist, secular.	4. Party is traditionalist, religious

Examples of Strong Distributive Pressure (A): Indonesia (until 1955), Burma (until 1959 at least), the Philippines.

Examples of Weaker Distributive Pressures (B): Thailand, and perhaps Malaysia (until 1967).

elections depends on its capacity to provide rewards for the lower tiers of its structure at a constant or even expanding rate.

A regime that is dependent on its particularistic distributive capacity is also unlikely to solve its financial dilemmas either by structural reform or by tapping new sources of revenue. Most conceivable structural reforms, such as land redistribution, would strike at the resource base of many patrons and are thus unacceptable to parties whose *policy* interests coincide with the desires of its dominant patrons. Such regimes also have a most difficult time raising revenue from internal taxation. A rise in direct taxation would threaten their base of support; and, in fact, they are notorious for the undercollection of revenues due them, since favors to their clients often take the form of either leaving them off local tax rolls or ignoring debts they owe the government. The Burmese peasants connected to U Nu's faction of the AFPFL, for example, were almost universally in default on agricultural loans they had received as party supporters. They assumed the loan was a gift for clientship and knew that a government dependent on their votes could scarcely press matters.

If this analysis is correct, regimes under intense distributive pressures will characteristically resort to budget deficits, especially in election years, to finance their networks of adherents. Their reliance on heavily instrumental and highly monetized patron-client ties will also make it difficult for them to avoid running down their foreign exchange reserves to maintain their strength at the polls. The division of expenditures within the budgets of such regimes should also reveal a heavy emphasis on distributive expenses at the local level. Empirical studies of budget distribution, budget deficits, and foreign exchange expenditures over time in parliamentary Burma, Indonesia, and the Philippines, when compared with similar statistics for nonparliamentary periods in these same countries, or say, with statistics from Thailand and Malaysia should confirm this prediction.[61]

Democratic regimes which must cater to the strong distributive pressures generated by their electoral clientele are thus particularly vulnerable to the vagaries of world prices for primary products on which their budgets depend. As long as the economy expanded and world prices were buoyant, they could afford the costs in public jobs, pork-barrel projects and loan programs to solidify and expand their huge patron-client network. But a stagnating economy or declining world prices threatened the entire structure they had pieced together, since it relied so heavily on material inducements and relatively little on affective ties. In this context it may be that the collapse of Korean warboom prices for primary exports was the crucial blow to democracy in Indonesia and Burma. The Philippines may narrowly have escaped a similar fate by virtue of their longer and more legitimate democratic tradition, as well as by their not having suffered as proportionately large a loss in foreign exchange. Malaysia was less vulnerable, since she had just become independent and her strong government faced only moderate distributive pressures, while the Thai military elite was even less reliant on its distributive performance. The political stability or instability of parliamentary forms in these nations in the late 1950s was thus strongly affected by the strength of distributive pressures fostered by these political systems in the mid 1950s.

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[61] Many of these data are not in a form that permits easy comparisons. Although budget deficit and foreign exchange figures seem to fit this pattern, statistical confirmation will have to await further research.

On Anarchism and the Real World: William Godwin and Radical England

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The rhetoric of anarchism is very much with us today, be it from Robert Paul Wolff in the academy or Jerry Rubin in the streets.[1] In its radical defense of freedom, this anarchism repudiates politics and the use of political means to achieve that freedom. This is the very essence of anarchist thought, and, indeed, its most salient distinction from other revolutionary doctrines. Freedom, the anarchist holds, cannot be realized through political methods and certainly not through popular or mass action. What replaces politics for the anarchist is either education or theater. If the former, then the path to utopia is conceived of as slow and laborious, successful only in the distant future. If the latter, then the new order of freedom is forever around the corner, instantly achieved through repetitive defiant deeds and romantic acts of self-assertion even unto death and annihilation. The one form of anarchist thought is passive, resigned, and critical of premature and precipitous action; the other is activist and precipitous in the extreme. Despite their differences, however, both forms of anarchist thought share a most important belief, a common conviction of superiority. Above the mediocre, the petty, the base, the dull, and the deceived, stands the anarchist of either stripe, convinced of his superiority of intellect or feeling. No surprise, then, that as political thought anarchism has traditionally been the expression of an intellectual or artistic elite.[2] Like Abbie Hoffmann, the anarchist has always stood apart from the "Pig Nation," which he sees so mindlessly unaware of its misery and mediocrity.[3] There is, one must insist, nothing new in this. For Emma Goldman, an earlier American anarchist, this elitism was itself the very essence of anarchism. Writing of Nietzsche, she insists, "(he) was not a social theorist, but a poet and innovator. His aristocracy was neither of birth nor of purse; it was of the spirit. In that respect Nietzsche was an anarchist, and all true anarchists were aristocrats."[4]

The masses, more often than not, have seen the anarchist for what he is, an arrogant, albeit sensitive, snob; and they have rewarded him accordingly. Herbert Read, England's most distinguished anarchist of this century, sadly laments this fate of the artist-anarchist at the hands of public opinion. He must "accept this thankless task: to stand apart yet to mediate." Society will never understand or love him, he must "drink with Socrates, the deadly cup."[5] But there may well be another dimension to the popular repudiation of the anarchist beyond mere distaste for those who stand apart. It could be informed by an astute realization that the purity and/or insanity of the anarchist often serves the conservative interests of the powers that be.

All of this rings true for the founder of modern anarchist political thought. Indeed, William Godwin (1756–1836) gives to anarchist thought its peculiar ambiance and its enduring characteristics. A radical theorist, Godwin despised politics and the pursuit of change through political activity. Less interested in theatrics than in rational education and enlightenment, he still speaks for a literary and intellectual elite. As such he was repudiated by the leaders of the popular movements in his day. Enshrined in the sacred texts of anarchist tradition is Godwin's plea of 1793 in his *Political Justice* for "the dissolution of political government, of that brute engine, which has been the only perennial cause of the vices of mankind."[6] Libertarian though this may sound, Godwin, despite his philosophical commitment to freedom, was by no means a friend of reform. This divergence between the radical theorist and the conservative practitioner is evident in his views on political action and revolution found in *Political Justice* and in his public performance two years later when he sided with Pitt in the repression of the English Jacobins.

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[1] Robert Paul Wolff, *In Defense of Anarchism* (New York: Harper & Row, 1970) and Jerry Rubin, *Do It!* (New York: Simon & Schuster, 1970).

[2] The history of anarchism both as movement and set of ideas has been ably presented by James Joll, *The Anarchists* (New York: Grosset and Dunlop, 1964) and George Woodcock, *Anarchism: A History of Libertarian Ideas and Movements* (New York: Meridien, World Publishers, 1962). Particulary useful as a contemporary indictment of anarchism as both elitist and counterproductive to the potential for change and reform in the real world is Benjamin R. Barber's excellent *Superman and Common Men: Freedom, Anarchy and the Revolution* (New York: Praeger, 1971), a position which this paper shares and hopefully corroborates.

[3] Abbie Hoffmann, *Woodstock Nation* (New York: Random House, 1969) and *Revolution for the Hell of it.* (New York: Dial Press, 1968).

[4] Emma Goldman, *Living My Life,* 2 vols. (New York, A. A. Knopf, 1931) I, 194.

[5] Herbert Read, *To Hell with Culture* (New York: Schocken, 1964) p. 9.

[6] William Godwin, *Enquiry Concerning Political Justice and Its Influence on Morals and Happiness* (Toronto, Univ. of Toronto Press, 1946) 11, 212. All future references to this work will appear with volume and page numbers in the text of this paper.

To facilitate an understanding of Godwin's anarchism and the extent to which it will mold the later tradition, the discussion here will be organized around five philosophical and political sections: (1) Godwin's destructive attack on government and institutions; (2) his positive vision of the anarchist society to replace "the brute engine government;" (3) his discussion of how to achieve this anarchist utopia, or, more importantly, how not to achieve it; (4) doctrine meeting practice, the clash between Godwin and the English Jacobins in 1795–1796; (5) speculations on the reasons for Godwin's break with the radicals and the significance of this for the future development of radical and anarchist thought. One final comment by way of introduction: Behind much of this paper are events and a scenario that will seem familiar to many readers; the 1790s were in England a time not unlike our own day. The response described here has continued to recur, so that even today we see the wasteful confrontation of elitist radicals, disdainful of politics, and popular radicals seeking political change.

The "Euthanasia of Government": Godwin's Attack on Law, Power, and the State

It is worthwhile at the outset to schematize the development of Godwin's argument. Two stages of destruction are followed by one of visionary reconstruction. The first negative stage involves an assault on the liberal tradition, carried out primarily by invoking Rousseau. Then follows the attack on law and political authority in the name of the liberal values of private judgment and individuality. There is, to be sure, some tension, incompatibility, and even contradiction, between these two destructive aspects of the argument, some of which remains and cannot be reasoned away. But much of this tension is resolved in the positive vision of anarchist society. The synthesizing agent is the principle of sincerity. In bare outline, then, these are the stages in Godwin's philosophy of anarchism. One can turn now in greater detail to each stage in the argument.

Godwin begins *Political Justice* with a root and branch attack on the Lockean natural rights tradition. His main charge is that it is too egotistical, too concerned with the individual's right to do as he wills. Sacrificed in this world view, according to Godwin, are duty, justice, and concern for the common good. Writers like Locke, he contends, in their "unprofitable disquisitions . . . respecting the probable origin of government," and in investigating rights, have failed to ask what form of government was the most conducive to public welfare (I, 123). Godwin repudiates any notion of contract based on some original promise. He does this by simply repeating the arguments of Hume (I, 188; 194–95; 219). What irritates him even more is the basic liberal notion of rights, for as far as Godwin is concerned rights "are all of them superceded and rendered null by the superior claims of justice." Man has no permissive grant to do as he wishes based on some supposed ethic of self-justification. He is bound by justice to do his duty, to employ his talents, his understanding, his strength, and his time, in the production of the greatest quantity of general good. One's duty is to see that his every act is "bound to . . . the general weal, that is, for the benefit of the individuals of whom the whole is composed" (I, 166; 135; 13).

Godwin is perfectly willing to dispense with the most fundamental of liberal rights. Man, he states, has no right even to his life, "when his duty calls him to resign it." Neighbors are duty bound to deprive one another of liberty and even life, he suggests, if to do so is absolutely necessary to the prevention of greater evil. Godwin sees such calculations of duty by and large as easy. It is, after all, blatantly clear that should one's mother and Archbishop Fenelon be trapped together in a burning house the latter should be saved. Not only has Godwin thus dealt with Locke and Hobbes, he also repudiates what will be one of Mill's major liberal contentions. Men have a duty to amend the errors of anyone they may see or know, suggests Godwin. "It is absurd to suppose that certain points are especially within my province, and therefore [that] he may not afford me, invited or uninvited, his assistance in arriving at a right decision" (I, 167; 162). In his assault on the liberal obsession with private rights Godwin reads very much like contemporary Straussian critics:

> According to the usual sentiment, every club assembling for any civil purpose, every congregation of religionists assembling for the worship of God, has a right to establish any provisions or ceremonies, no matter how ridiculous or detestable, provided they do not interfere with the freedom of others. Reason lies prostrate at their feet; they have a right to trample upon and insult her as they please . . . but as it has been before shown that it

> cannot be their duty to do anything detrimental to the general happiness so it appears with equal evidence that they cannot have a right to do so. There cannot be a more absurd proposition, than that which affirms the right of doing wrong (I, 165–66).[7]

Like Rousseau, his exemplar in much of this criticism, Godwin is basically a *philosophe;* and the Enlightenment thought he espouses in essence revives Platonic and Stoic conceptions of right reason, truth, and justice. Indeed, at this point there is every good reason to expect Godwin to continue his Rousseauean assault on liberal government by repudiating its negativism, its failure positively to promote virtue, the good life, or at least the general good. All he has written to this point in the argument tends in that direction. If not an enlightened despot then at least a democratic sovereign finding duty and right reason via the law of the general will seems in order. But Godwin follows neither of the two alternatives taken by the French *philosophes;* he sets out on his own path to anarchism. Is it the duty of governments to watch over the manners of the people, he asks? Ought it to superintend the sentiments of the people and "encourage such as are favorable to virtue, and to check in the bud such as may lead to disorder and corruption?" "Surely," he states ironically, "government can do some positive good" (II, 214–15). But no, alas it cannot, he proclaims. Helvetius and Rousseau are both rejected, and in an abrupt about face there is the Godwin one expected.

> Government is, in all cases, an evil; it ought to be introduced as sparingly as possible. Man is a species of being, whose excellence depends on his individuality; and who can be neither great nor wise, but in proportion as he is independent (II, 214–15).

This turn in the argument introduces Godwin's second destructive stage, his assault on law and political authority in the name of private judgment and individuality. The mood shifts decisively and one finds the traditional liberal preoccupation with individual freedom pushed to extremes—to anarchist extremes. But the antiliberal Godwin, the critic of rights and the defender of duty and a higher public good, will eventually return. He will eventually proclaim a vision of an ideal society, where not governments but friends and neighbors will lead men to truth and virtue. But in this early stage of the argument he is concerned with the defense of private judgment and the need to replace the brute engine, government.

Godwin argues, then, that self-determination and independence are basic to the nature of man. Free man consults his own reason and draws his own conclusions. He then will conscientiously conform to whatever are his own ideas of propriety. The free man is one who exercises his own judgment and, "stands by himself, and rests upon his own understanding." The fundamental principle of politics, writes Godwin, is the universal exercise of private judgment, a doctrine "unspeakably beautiful." Each man seeks truth and right through his own rational faculties. The only principle which legitimately imposes upon him any specific conduct is the conviction of his individual understanding (I, 168; 182; 181). We are unmistakably in the camp of individualistic liberalism. It is only necessary for Godwin now to push this liberalism to the finality of anarchism. This he does by criticizing first governmental authority, then law, and finally punishment.

Anything that moves man to action other than his own private judgment, Godwin contends, is by definition force or coercion. Government, which sets up other men as permanent arbiters over the actions of individuals, is the ultimate determinant of most men's destinies. As such "government is nothing but regulated force; force is its appropriate claim upon your attention" (I, 230). But Godwin's rejection of government is much more complex than this often quoted passage. Government is only the third of three possible forms of authority. In the first, the authority of reason, the individual obeys simply himself. Reason, indeed, represents the absence of government. The second form of authority is confidence in and deference to some esteemed figure and his decisions. As Godwin rightly points out, this is the original meaning of the word authority; it represents "compliance which is the offspring of respect" (I, 231). Godwin regards only the third form of authority as genuinely political; this is governmental authority, to which compliance is based not upon esteem and respect but upon force and power. Self-government, the first form of authority, is obviously the ideal. Of the two forms of external authority, the second is clearly preferable to the third, according to Godwin, especially when the individual has reason to believe that another person knows better than he what is proper to do. If this is the case, then one ought to conform to such direction. The door is opened here, we must note now and return to later, for an elitist appreciation of enlightened tutelage, an idea not at all uncongeial to the eighteenth century *philosophe*. But, othing, Godwin writes, can justify the third orm of authority. It is totally contrary to reaon and justice to surrender one's private judgnent, i.e., to obey another man because he is uperior in station "or because of concurrence f circumstances has produced him a share in he legislative and executive government of our ountry." Men need not, however, take arms to verthrow this brute engine, that Godwin calls overnment. Such forms of government are loomed anyway, writes Godwin, for all governnent will ultimately wither away. People obey overnment only because they are ignorant. When knowledge and virtue increase, men will liscover that there is "no mystery in governnent which uninitiated mortals must not preume to penetrate." At some point they no onger will need nor will they tolerate authority f governments, the third form of authority. Formal governments will be abandoned and nly the authority of the truly esteemed and wise will operate. But as knowledge and virtue urther increase men will be themselves wise nd defer to the authority of no one but themelves. With the inexorable passage of time, hen, and with the progressive dissemination of nowledge and wisdom all forms of governnent, formal and informal, will slowly be put o rest.

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[7] Compare, for example the sentiments expressed in this passage with those of Walter Berns in his *Freedom, Virtue and the First Amendment* (Baton Rouge: Louisianna State Univ. Press, 1957.)

> n other words, government cannot proceed but pon confidence, as confidence on the other hand annot exist without ignorance. The true supporters f government are the weak and uninformed, and ot the wise. In proportion as weakness and ignornce shall diminish, the basis of government will lso decay. This, however, is an event which ought ot to be contemplated with alarm. A catastrophe f this description would be the true euthanasia of overnment (II, 214–215).

Having disposed of governmental authority, Godwin turns next on law. None of Rousseau's hapsodic appreciation of law is found here.[8] Godwin's central point is that the diversity of uman experience defies generalization, one mportant form of which is abstract law. No ction of any one man is the same as another's. Law is either stretched on a procrustean bed to it new cases, or else a new general law is made. n either case, Godwin argues, instead of being ertain and eliminating ambiguity, law is loaked in uncertainty. Few can thus know what to expect at the hands of the law. Morever, it "fixes the human mind in a stagnant ondition," and substitutes permanence for progress (II, 400; 403). Because of these shortcomings law must be abolished and replaced by what Godwin calls situational wisdom. Reason, he holds, must be applied anew to each incident, and all the many and unique factors of each case must be heard and understood. Only this conduct befits a rational people.

The final step in the destructive phase of Godwin's anarchism is his attack on the doctrine of punishment. It is essential for his anarchism that he question the legitimacy and the utility of punishment, since, as he himself concedes, the most important justification for government is that in order to prevent the hostile actions of one member of society upon another there must be the sanction of criminal punishment. Punishment is unjust, however, Godwin contends, because it assumes free will when in fact necessity and circumstance propel the criminal to his action. In a famous passage Godwin suggests that the assassin can no more prevent the murder he commits than can his dagger (II, 324). Moreover, he adds, punishment is a useless method of dealing with a wrongdoer. Like all forms of political coercion it is a mere recourse to force that has no intrinsic capacity to convince. "It is no argument . . . it begins with violently alienating the mind from the truth with which we wish it to be impressed." One ought to reason with a criminal and convince him of his errors, for as Godwin firmly believes, no man can reject truth and choose falsehood when the evidence is clearly and fairly presented (II, 237; 225).

So much, then, for the destructive phase of Godwin's anarchism. Government, law, and coercion are pushed aside by self-determined, wise men who use their own private judgment and their own reason. These men are independent and autonomous. "Each man should be wise enough to govern himself, without the intervention of any compulsory restraint." So keen is Godwin on protecting this individual autonomy that he pushes his anarchic-individualist ideal to the elimination of all forms of cooperation. "Everything that is usually understood by the term cooperation," he writes, "is in some degree, an evil" (II, 246; 501). And that degree is the extent to which cooperation inhibits a man's individuality and prevents his thinking for himself. Meals in common and communal labor are suspect. Men may now have to work together in factories, but technology, will soon, he predicts, make the most extensive operations within the reach of one man. Theatrical and musical performances will go, too. Autonomous men may very well cease to "repeat words and ideas that are not their own," or music composed by others. Though they may find pleasure in one another's company, solitude will be the greatest delight of autonomous men: they "ought to be able to do without one another." And so one comes to Godwin's famous views on marriage, which he sees as both unjust and monopolistic. Even more harmful is its unnecessary emphasis on cooperation and its infringement on personal independence (II, 503; 504; 505; 508ff.).

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[8] See for example Rousseau's praise of law "the elestial voice," in his *Discourse on Political Economy* New York: E. P. Dutton & Co., 1955), p. 294.

In these prophetic passages of *Political Justice* one finds the quintessential statement of Godwin's anarchic individualism—the ideal of independent, self-determined, non-cooperative man. This is the often parodied Godwin that has amused his detractors ranging from the staff of the *Anti-Jacobin Review* of his day to the Victorian Leslie Stephen. But Godwin's anarchism, and anarchist thought after him, does not end with this longing for individual privacy and autonomy. It moves on to a second stage, more positive and less liberal—the vision of an ideal order, in which emphasis shifts dramatically to values of community, cooperation, and solidarity, all achieved through the coercion of virtuous and duty-seeking neighbors.

"Political Simplicity": The Politics of Utopian Anarchism

Godwin's vision of anarchist society is structured around three basic principles: "political simplicity," "public inspection," and "positive sincerity." One must begin with "political simplicity," for the two other principles presume its prior existence. The practice of sincerity and the performance of public inspection require the geopolitical reorganization of society. The masses of mankind have been duped, claims Godwin, "by the mysterious complicated nature of the social system" (II, 208); the social order must therefore be simplified. Simplicity of political arrangement would liberate men and could be achieved through localism and federalism. If nations were broken down into smaller autonomous units, parishes that regulated their own affairs, they would cease their tyranny:

> The ideas of a great empire and legislative unity, are plainly the barbarous remains of the days of military heroism. In proportion as political power is brought home to the citizens and simplified into something of the nature of parish regulation; the danger of misunderstanding and rivalship will be nearly annihilated. In proportion as the science of government is divested of its present mysterious appearances, social truth will become obvious, and the districts pliant and flexible to the dictates of reason (II, 293).

A general assembly of these parishes woul be a rare and extraordinary event. Normally representatives of the parishes would assembl but once a year to adjust their few differences This assembly would pass no laws, take n votes. To do so would be an affront to reaso and justice, "the deciding upon truth by th casting out of numbers" (Rousseau is not to tally forgotten). As part of the generally nos talgic flavor of Godwin's ideal order, thi yearly assembly would merely perform the me dieval task of hearing complaints and represen tations from constituents (II, 205; 207).

Within the parish no political institution would be necessary. Occasional disputes woul be settled by calling a jury, but these jurie would have no coercive power. They woul only invite and reason. They would persuade not dictate. Political authority in the paris would be replaced by public persuasion. Th entire system is made to work by the power "o public inspection." In a simple and small un the "observant eye of public judgment" woul prompt each individual to seek the commo good. Godwin was not the first, nor the last, t suggest that men are less moral under the cove of a large society and more virtuous in th face-to-face contacts of simpler and more loca surroundings. Extensive governments requir coercion and force; smaller units allow th sway of opinion. "The inspection of every ma over the conduct of his neighbors, when un stained with caprice, would constitute a censor ship of the most irresistible nature." Law an government are replaced, then, in Godwin's an archist utopia, by the watchful and censorin eye of public opinion. The disapprobation o one's neighbors is enough, he holds, to mak everyone shy away from vice. (II, 211; 231 274).

Public inspection and censorship woul work, however, only if neighbors tell the trut to one another; i.e., only if they are sincere Godwin's preoccupation with sincerity, hi quaint insistence that one must not have hi butler lie when one is in but not eager to se company, is more than simply the eccentricit of a bookish crank. It is, in fact, a principle ba sic to his vision of a new order, for public per suasion can only work if each citizen is hi neighbor's "ingenious censor" and if they pro claim one another's "virtues, good deeds, mean ness and follies." Only through such just an impartial evaluation can vice presumably b discouraged and virtue encouraged. In the tell ing of these truths, neighbors should not be re strained by private protections like libel laws The occasional false charge or mistaken censor

Godwin insists, is a small price to pay for the advantages of unlimited and sincere criticism (I, 329; II, 270ff.). It is in light of this important political purpose, then, that one is to understand Godwin's claim that sincerity is "the most powerful engine of human improvement." But sincerity does more: it helps bring together the antiliberal and the liberal aspects of his anarchism. Liberated from govermental coercion, men in Godwin's parishes will freely exercise their private judgments, but they will not leave each other alone. They will not till their gardens in passive liberal enjoyment of private rights. They will recognize their fundamentally nonliberal duty, "the most indispensable business of man, to study and promote his neighbors welfare" (I, 340; 339). Their exercise of private judgment will be in the service of others. It will pursue a higher will, a higher realm of truth. This pursuit will take the form not of legislative enactment of a general will but rather of positive, constructive, and sincere neighborly advice and criticism. There is, according to Godwin, no plurality of ideals. The nature of man and the nature of his good life are not relative. "There is but one perfection to man; one thing most honorable . . . all else is deviation and error; a disease to be cured, not to be encouraged." That single and overriding ideal is "benevolence," or as he calls it elsewhere, "disinterested transport." The goal of man's social existence is "to make others free, virtuous and wise." Godwin is convinced that "there is but one best mode of social existence," which can be deduced from the principles of human nature for there must be one code of truth on the subject of our reciprocal duties (I, 240; 314). It is as if Rousseau's legislator and general will were replaced by what Godwin calls:

> Putting ourselves in the place of an impartial spectator, of an angelic nature,—beholding us from an elevated station, and uninfluenced by our prejudices, conceiving what would be his estimate of the intrinsic circumstances of our neighbor and acting accordingly (I, 133).

Each individual is obliged to see that his neighbor lives up to the uniform code of truths which teach benevolence and public spirit. To do this, he must, however, break the cold reserve that keeps men at a distance from one another. Men must end the charade in which they talk to one another without disclosing their feelings or opinions. They must sincerely and openly speak to each other about their moral character, "how they ought to be employed, and how to be improved" (I, 294).

Sincerity is the key, then, to understanding the relationship of the diverse parts of Godwin's argument, as well as being the essential value in his simple, face-to-face, nonpolitical society.[9] Laws would be unnecessary, for example, in this vision of political simplicity. The inhabitants of a small parish seeing that laws were irrational would judge each case not by written axioms but by the circumstances and demands of the particular case, what Godwin calls the "spontaneous justice" of the parish. But even this would become unnecessary at some point, for eventually crime would be totally eliminated. Public disapproval would turn all knaves honest. Who would commit a crime when faced by "the sober condemnation of every spectator?" Even the "legal" crime and murder of governments would come to an end in the new order, for they were indelibly associated with the false notions of extensive territory and dreams of glory, empire, and national greatness (II, 294; 410; 275; 361).

The new anarchist order would see the decline of crime for reasons much more basic than simply the public disapproval of knaves. The very motivation to crime would dry up. For, according to Godwin, its most important source was economic inequality. If this were changed, crime would cease. Godwin is dramatically outspoken in his condemnation of inequality. Few English Jacobins of the 1790s would outdo his indignation at social injustice; perhaps only his wife, Mary Wollstonecraft, and Tom Paine could match him on this score. The rich, Godwin claims, were "in all countries directly or indirectly the legislators of the state." Their laws invariably favored their interests over those of the poor and "reduced oppression into a system" (I, 18; 19). While the rich, for example, could associate for monopoly and gain, the law of Parliament prevented workingmen from combining in unions. The effect of this inequality on the poor was disastrous. To be born poor, Godwin writes, "may be said under another name to be born a slave." Any potential for distinction and intellectual achievement in the lower orders was dashed by "the present ordering of society . . . the great slaughterhouse of genius and of mind."[10]

The great ally of the rich in this oppression and brutalization of the poor was, according to Godwin, modern technology; thus he sets the pattern for subsequent generations of anarchists in the condemnation of the machine age. Like many of them, however, he also relished the progressive potential of technology. Would not, for example, the drudgery and oppression of communal factory work be ended with the invention of one great machine operated by a solitary individual? But his response in this, his antitechnological, mood was in fact a rather acute anticipation of later notions of alienation, for the end product of the factory system, he argued, was the estrangement of man from himself, his transformation from worker to machine.

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[9] Godwin's concern with sincerity is also important in understanding his link to Romanticism. Here and in his discussion of openness, spontaneity, and values of the heart (II, 280; I, 335–340) one sees a Godwin far different from the stern rationalist he has usually been depicted as, and much closer to the romantic so evident in his novels.

[10] William Godwin, *The Enquirer* (London, 1797), pp. 62, 17.

> A mechanic becomes a sort of machine; his limbs and articulations are converted, as it were, into wood and wires. Tamed, lowered, torpified into this character, he may be said perhaps to be content.[11]

The most grievous political result of this constant and unmerciful exercise of superiority by the rich was, according to Godwin, that it produced crime, which in turn could be used as justification for punishment and, therefore, as rationale for government. The exploitation by the rich led the poor to seek reprisals. The poor see "the state of society as a state of war." In his novel *Caleb Williams,* Godwin has his pathetic hero, driven to crime, exclaim in his defense:

> Oh poverty! Thou art indeed omnipotent! Thou grindest us into desperation; thou confoundest all our boasted and most deep rooted principles; thou fillest us to the very brim with malice and revenge, and render us capable of acts of unknown horror.[12]

The inequality and injustice of this economic system would disappear in Godwin's simple anarchist society. The factory, taxation of the poor, legislation by the rich, oppression and misery leading to class crime—all these would be replaced simply and magically in the transition to a frugal agrarian economy. Once again Godwin anticipated generations of future anarchists. Taxation would be unnecessary in the parish, he holds, since there would be no foreign wars, no domestic office holders. The usual army of government functionaries, clerks, collectors, etc. would not be needed. But Godwin felt that the right to private property, on the other hand, was still legitimate, and was in fact, essential for the exercise of private judgment and independence. Too great and too unequal an accumulation of property was, to be sure, unjust, but the small and simple parish society would restrain this accumulation by "the good sense of the community, and the inspection of all exercised upon all." Eventually the parish would see an equality of holdings with no man having for his personal use more than his necessities required. Man in his parish society would work only in simple manual industry and agriculture. No one would accumulate unearned property produced by the labor of others; "the mathematician, the poet, and the philosopher, will derive a new stock of cheerfulness and energy from the recurring labor that makes them feel they are man" (II, 315, 433; 412; 480; 482). No one in Godwin's utopia would manufacture luxuries, however, an unnecessary chore that occupied many men in modern nations. Whereas the object of modern society was to multiply labor, Godwin's parish economy would simplify it, would produce "a state of the most rigid simplicity." In the England of the 1790s, one in twenty worked on agriculture, he conjectured; but in his utopia all men would work in the fields at some time. All other jobs would be eliminated. The job of supplying the community with the absolute necessities of life would be parceled out to all men and thus occupy one-twentieth of every man's time. Each member of the community would perform manual labor about half on hour a day; the rest of the day he would enjoy life, learning, and nature in his idyllic rural paradise (II, 480). More probably, I suspect, he would spend much of his free time inspecting and giving sincere advice to his neighbors.

Much of the imagery of Godwin's visionary parish society was familiar to Englishmen of his day. The rural nostalgia and the longing for the simplicity of a frugal economy and a face-to-face society were themes often found in earlier opposition writings under Walpole, both from Tories, like Bolingbroke and Gay, and radicals like Trenchard and Gordon. These themes are also writ large in the Toryism of Goldsmith's *Deserted Village* and the radicalism of Southey's *Wat Tyler.*[13] Godwin, in his novel *Imogen,* written some nine years earlier, had himself depicted an idyllic community of Druids with no government, and no ranks of distinction. His sincere and simple protagonists, Imogen and Edwin, roundly overcame the villain Roderick. But the vision of *Political Justice* differs from other nostalgic pastorals and from the spate of English imitations of Rousseau in its transcendence of mere social and economic nostalgia and its thoroughgoing political radicalism—in short, in its complete dismissal of the political.[14]

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[11] William Godwin, *Fleetwood* (London, 1805), I, 277.

[12] William Godwin, *Caleb Williams,* (London, 1794), II, 30.

[13] See Isaac Kramnick, *Bolingbroke and His Circle, the Politics of Nostalgia in the Age of Walpole* (Cambridge: Harvard Univ. Press, 1968) and Bernard Bailyn, *The Ideological Origins of The American Revolution* (Cambridge: Harvard Univ. Press, 1968).

Even here, however, one wonders if perhaps Godwin didn't have a model for his vision. Where else, for example, might he find a vision of a community free of law, free of coercion, free of institutions, free of power, indeed free of politics? Where else might he find a community of spontaneous solidarity and zealous regard for one another's moral well being? The answer may well be that Godwin, the former dissenting minister, could never quite free himself from the anarchic vision of the primitive Christian community—but that is another story. Suffice it to point out here that Godwin held no illusion that if the government of Britain were dissolved in 1793 his simple parish society would immediately flourish. The citizenry were still unregenerate, and not fit for this dispensation; it would come only in due time. Before one speculates on this eventuality, one must make clear how Godwin felt this dispensation would *not* come about. The radical ideal of anarchic policial simplicity, he was certain, could never be brought about by radical political agitation or reform.

"Tumult and Violence": Godwin's Critique of Radical Politics

In the preface to the first edition of *Political Justice* Godwin credits many of his insights to the recent experiments in America and France. He was persuaded "of the desirableness of a government in the utmost degree simple" by the ideas suggested in the French Revolution (I, x). That event, he confesses, more than anything else, prompted his writing the *Enquiry*. Like Price's Sermon at Old Jewry, Paine's *Rights of Man*, Mackintosh's *Vindiciae Gallicae*, and Wollstonecraft's *Vindication of the Rights of Women*, Godwin's *Political Justice* was part of the literary outburst produced among English Jacobins by the French Revolution. But Godwin's response has very much the flavor of another written in those years, Burke's *Reflections*. Godwin, the philosophical radical, would have no truck with any political action designed to encourage or hasten the achievement of anarchism. The Revolution in France, to be sure, had awakened Godwin's love for freedom, but it had also irredeemably frightened the calm philosophical skeptic in him. Thus in *Political Justice* Godwin specifically repudiates reform politics.

Tumult and violence, the phrase he habitually used to describe the hurly-burly of radical politics, could never bring about an ideal order, he argued. Only when the great majority of society were persuaded, would "the chains fall off of themselves." Any effort to introduce political change—no matter how excellent theoretically—that did not consider the state of the public mind would be both absurd and injurious. Men ought to speculate on the ideal form of political society, Godwin felt, but they should be on guard against precipitate measures. Ideals could be securely established only when there was a general preference in their favor. Philosophers might well know that one form of government is most desirable, but inferior forms of government must still be tolerated since immediate efforts to remove them would introduce something far worse, chaos and turbulence. All the wheels, he writes, must move together lest the fragile structure of society break down:

> The interest of the human species requires a gradual, but uninterrupted change. He who should make these principles the regulators of his conduct, would not rashly insist upon instant abolition of all existing abuses . . . Truth, however unreserved be the mode of its enunciation, will be sufficiently gradual in its progress. It will be fully comprehended, only by slow degrees, by its most assiduous votaries; and the degrees will be still more temperate, by which it will pervade so considerable a portion of the community, as to render them mature for a change of their common institutions . . . we shall have many reforms, but no revolutions . . . Revolutions are the produce of passion, not of sober and tranquil reason (I, 99; 242ff.).

Political change will come not through political action but through knowledge. New or reformed laws or policies would change nothing if opinion and understanding remained the same. The proper task for the friends of humanity was not to introduce new practices in politics, but "enquiry, communication, and discussion." Godwin assailed Rousseau on this very point. He felt that Rousseau's recourse to a civic religion in the effort to reorient the public mentality enlisted a new authority over a people not yet capable of perceiving the wisdom of the new order. Duping men to receive a system whose reasonableness is not a part of their understanding is, Godwin suggests, "a questionable method for rendering them sober, judicious, reasonable, and happy." Burke, too, is criticized on this score for his suggestion that men cherish their prejudices. Lulling men not to use their own understanding is wrong when used to uphold the established order as well as when used to change it (II, 128; 130; 140).

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[14] William Godwin, *Imogen, A Pastoral Romance* (London, 1784, reprinted 1963, New York Public Library, edited by J. W. Marken). For the English Rousseaueans see J. A. Warner, "The Reaction in Eighteenth Century England to Rousseau's Two *Discours*" in *P.M.L.A.* 38 (1933) and Michael Sadler, *Thomas Day, An English Disciple of Rousseau* (Cambridge: Cambridge Univ. Press, 1928).

The major thrust of Godwin's caution, however, is directed against the "domestic spoilers" who preach immediate dissolution of all governments. Godwin contends that if this should occur in 1793, in a period when political truth had not yet permeated the understandings of all, an anarchy of barbarism and violence would follow, not the reasoned anarchy of political simplicity, positive sincerity, and public censorship. The resultant insecurity and disorder would be far worse than the preceding despotism. Particularly vulnerable were not the poor and the exploited, but men the very likes of Godwin, "the wisest, the brightest, the most generous and bold." In a state of barbaric and violent anarchy one "must bid farewell, to the patient lubrications of the philosopher, and the labor of the midnight oil." True anarchy would not result from headlong and impatient action:

> In order to anarchy being rendered a seed of future justice, reflection and enquiry must have gone before; the regions of philosophy must have been penetrated, and political truth have opened her school to mankind (II, 366; 368; 371).

Godwin is so outspoken in his opposition to any form of resistance or revolution that he even rejects the argument from Locke so often cited by English radicals. Nations have no right, Godwin answers, to shake off governments simply because of an alleged break with a social contract or usurpation of traditional rights. Since government is founded on opinion, he argues, even bad governments must have come to be accepted by whatever be the current level of popular understanding. If the people are unenlightened and unprepared for a state of freedom, then resistance is inappropriate, and if political knowledge has spread among them, then their resistance is unnecessary, for subjection of the people even by foreign forces will be futile and short-lived. The theme is constantly reiterated: "if the nation be not right for a state of freedom" no resistance is justified (I, 252ff; 302).

Godwin is careful in *Political Justice* to remind his readers that "the great cause of humanity" has two enemies, the Burkean friends of prejudice and antiquity, on the one hand, and the "friends of innovation" on the other. Having warned of two enemies he concentrates, however, on only one, those "impatient of suspense, inclined violently to interrupt the calm, incessant, rapid, and auspicious progress which thought and reflection appear to be making in the world." He even uses the misgivings of the one enemy to combat the other. Like Burke, Godwin keenly saw the paradoxes inherent in the Jacobin-Revolutionary mentality. Resisting tyranny, the revolutionaries became tyrants themselves, scrutinizing men's thoughts and punishing their opinions. "To dragoon men into adoption of what we think right," Godwin writes, "is an intolerable tyranny" (I, 256; 257).

Much like an orthodox Marxist, Godwin, in the next stage of his attack on radical reform, cautions against rushing the inevitable developments of progressive change. "There is a condition of political society best adapted to every different stage of individual improvement." Everything is in order, and succeeds at its appointed time. History must unfold in its predetermined stages, one cannot impatiently hurry it to its conclusion. The historical passage is long and arduous;" and if we aspire to the final result, we must submit to the portion of misery and vice, which necessarily fills the span in between," he writes. One such evil is the unfair distribution of wealth. This, too, cannot be changed by premature violence or even legislative action; it can only come from a "revolution of opinions." Men should be careful lest they rip down too hastily the institutions of property and its inequalities which are so woven into the social fabric. If private property were suddenly abolished, or if even the principles of feudal rights and privileges of rank were instantly dissolved, thousands of men, indeed whole classes, would be reduced to immediate wretchedness, Godwin contends, and all manner of calamity, convulsion, and upheaval would ensue more dreadful than the evil remedied. It is Godwin the proto-Marxist historicist again who writes, "the inequalities of property perhaps constitute a state, through which it would be necessary for us to pass" (II, 372; 441; 418; I, 273, 259). It is more than simply anachronistic, however, to label Godwin thus. His historical caution on this score is, after all, informed by fundamental bourgeois convictions.

> Without permitting to every man, to a considerable degree, the exercise of his own discretion, there can be no independence, no improvement, no virtue and no happiness. This is a privilege in the highest degree sacred; for its maintenance no exertions and sacrifices can be too great. Thus deep is the foundation of the doctrine of property. It is, in the last resort, the palladium of all that ought

o be dear to us, and must ever be approached but with awe and veneration (II, 450).

Burke might well agree with Godwin's criticism of the politics of resistance, revolution, and redistribution, but Burke would never agree with Godwin's more basic and unique assault on political associations, parties, and political life in general. Political parties, associations —any form of cooperative political endeavor—all fall before Godwin's ruthless philosophical bias. Sounding much like a contemporary student of group politics, Godwin insists that in any such collective gathering the intemperate and artful push aside the "prudent, the sober, the skeptical, and the contemplative." Truth could never emerge from the superficial convictions of committed partisans; it would come only from rigorous and laborious enquiry. Party, Godwin argues, puts a premium not on thinking for one's self but on identifying one's creed with that of one's associates. There is no encouragement to enquiry when all the diversity of opinion has been reduced to the position of one common mass. Political meetings encourage harangue and declamation, he contends, which lead only to passion and not argument. Orators and party leaders work on the passions of their followers, playing on hopes and fears. "Truth can scarcely be acquired in crowded halls and amidst noisy debates" (I, 290).

Burke need not have feared, different as their opinions on party were, for in these passages Godwin was not criticizing the Rockingham Party nor the Whigs, old or new. His clearly intended victims were the radicals, those who thought that politics and not philosophy would usher in the new political age.

The Philosopher and the Agitator: Godwin's Attack on John Thelwall

This theoretical repudiation of activism found in the bible of anarchism, Godwin's *Political Justice,* was soon tested in the arena of Jacobin politics. In 1795 and 1796 Godwin's philosophical anarchism and the politics of radical agitation met head on. The second party to the confrontation was the London Corresponding Society and its leader John Thelwall.

The London Corresponding Society, founded in 1792 by a shoemaker, Thomas Hardy, hardly seems to merit Burke's assessment as "the mother of all mischief."[15] Its membership, according to France Place, an early official of the Society, was about two thousand; more generous estimates place it around twenty thousand. The social types attracted by the Society were artisans; mainly hatters, bakers, grocers, booksellers, and shoemakers—"the thinking part of the working people"— as Place characterized them.[16] The division meetings of the Society were devoted to political discussion and communication with other radical societies, often with the two very large ones in Sheffield and Norwich. Pamphlets and broadsides were periodically published proclaiming the Society's positions on issues of the day. The ideology of the group was simple and straightforward and was shared by most of the Jacobin groups of the 1790s. It stood for three principles: annual parliaments, universal suffrage, and parliamentary reform. Despite a liberal dose of Paine's ahistorical and nonconstitutional argument from nature and natural rights, most of the Society's rhetoric was still couched in terms of recapturing ancient lost English rights. Communications and pamphlets were as often signed "Anglo-Saxon" as "citizen." The ancient constitution, freed from its "Norman Yoke" was authority enough on which to base true English liberty in the three areas of reform. There were in the Society a very few who, according to Place, "were desirers of confusion, and for all sorts of absurd and violent measures." Some members, for example, accepted Paine's Republicanism, but even they assumed that it would be achieved after the House of Commons was reformed and the King and Lords set aside peacefully.[17] The L.C.S. (as it was known in its day) was, in short, an association of sober shopkeepers, craftsmen, and journeymen. Some indication of the extent of its threat to the social order can be gathered from Place's evaluation years later of his association with the Society. It had a profound moral impact on its artisan membership, he wrote. Instead of wasting time in the public house, they were encouraged to read books, to think for and respect themselves. In addition, Place wrote, it encouraged them to educate their children. Lest this general assessment be doubted Place related how in 1822 at an anniversary dinner for Hardy he saw twenty-four men whom he knew at the Society in the middle of the 1790s. "Of the twenty-four, twenty were then journeymen or shopmen; they were now all in business and all flourishing men."[18]

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[15] Thomas Hardy, *Memoirs* (London, 1832), p. 109.

[16] Add. Mss. 35, 143 f/9. All references to Additional Mss. refer to the manuscript collection of the British Museum. For further details on the Society see C. B. Cone, *The English Jacobins* (New York: Scribner's, 1968); Philip Anthony Brown, *The French Revolution in English History* (London: G. Lockwood and Son, 1918); W. P. Hall, *British Radicalism 1791–1797* (New York: Columbia Univ. Press, 1912); G. S. Veitch, *The Genesis of Parliamentary Reform* (London: Constable & Co., 1913); and E. P. Thompson's *The Making of the English Working Class* (New York: Random House, 1963).

[17] Add. Mss. 35, 143 f/90. (Place Papers)

How surprising then that in May, 1794, Pitt's government should try the leaders of the L.C.S. for treason! The great treason trial of that year brought to an end the first stage of Pitt's repression of the radical Jacobin community. It had begun over a year earlier with his decision to ban Paine's *Rights of Man,* and with Paine's subsequent flight and trial *in absentia.* Anti-Jacobin societies quickly spread across England and Scotland, as did John Reeves's Church and King Associations,[19] groups heavily assisted by Pitt's Government. Moreover, in 1793, the French King was killed, and England went to war. The blissful dawn that had so moved Wordsworth had turned to dusk. The campaign of repression was so widespread that few were surprised in May of 1794 when Pitt accused the Corresponding Society and other leading radicals of plotting:

> To assemble a pretended convention of the people, for the purpose of assuming the character of a general representation of the nation, superseding the representative capacity of the House and arrogating the legislative power of the country at large.[20]

The Tory newspapers echoed Pitt's charge. "The whole system of insurrection lay in the monstrous doctrine of the Rights of Man, and the Corresponding Society composed of the meanest and most despicable of people."[21] Godwin knew most of the accused. Three of them—Jerrald, Thelwall, and Horne Tooke—were good friends, One defendant, Thomas Holcroft, the writer, was Godwin's close friend. Nevertheless, Godwin had little love for the Society. It stood condemned of all he had written about political associations. Moreover, in *Political Justice* he had even criticized those who argued for change in the name of antiquity, having heaped ridicule on "those governed by the dicta of their remotest ancestors." In his book he had criticized the narrowness of groups like the L.C.S., groups that opposed "a specific tax perhaps, or some temporary grievance" (I, 245; 255). He had, after all, proposed a truly radical restructuring of society, albeit not readily attained. Friendship and the conviction that Pitt's charges were based not on actions but on publications and speeches won out, however, and Godwin anonymously published an attack on the court. Shortly thereafter, the defendants were found innocent by the jury, and at least one of them, Horne Tooke, credited this to Godwin's *Cursory Strictures on the Charge Delivered by Lord Chief Justice Eyre to the Grand Jury.* Soon, however, the basic differences between Godwin and the L.C.S. would surface, and true to his own creed of outspoken and sincere censorship no amount of friendship would quiet Godwin and prevent the confrontation within the Jacobin family.

The essential ingredient for the evolution of the controversy was the emergence after the trial of one of its defendants, John Thelwall, as the leading figure in the L.C.S. He gave to the Society a new and decidedly more radical face. Poet, playwright, pamphleteer, and politician, Thelwall cut a fascinating figure in the 1790s. Friend and confidant of Coleridge, author of Rousseauean pastorals, publisher of a weekly radical newspaper, and ever present agitator at public meetings and lecture halls, Thelwall may well deserve a place in history as the first great leader of the English working class.[22] His ideas are not our central concern here; in this story they are less important than his actions. Suffice it to say, however, that he was an intellectual disciple of Godwin's. In his *Tribune* articles and in his very popular *Rights of Man* (1796) he, too, was a perfectionist, who wrote on necessity, the fraud of punishment, and hypocrisy of patriotism, marriage, and gratitude, and one who recognized the obligations of sincerity and truth over friendship. He refused, however, to follow Godwin into the utopia of "political simplicity." Much more concerned with economic and social questions, he wrote primarily of specific reforms required to improve the lot of the working man. As one historian puts it he took "Jacobinism to the borders of Socialism." He also took it "to the borders of revolutionism," which is much more our concern.[23] Thelwall roused the L.C.S. from the depths of its despair and responded to Pitt and the legions of Church and King. His tactic was twofold: first, he gave rousing lectures with great rhetorical flourish, lectures which soon became the source of inspiration for scores of militant Jacobins. Much more courageous was his second tactic, the mass meeting; he organized and spoke to meetings at least three of which, in 1795 alone, drew more than 100,000 people to London.[24] Whatever his cause—universal suffrage, annual parliaments, shorter working hours, or redistribution of wealth—he brought to it an intensity and activism that pushed the L.C.S. from moderation to agitation and active intervention in the street politics of London. As an activist he was disdainful of philosophical and literary fellow-travelers like Godwin who refused to participate in Jacobin agitation.

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[18] Add. Mss. f/93. E. P. Thompson suggests, it should be noted, that Place may be glossing over its real radical thrust to fit his own later, more moderate position (p. 153).

[19] The full name of Reeves's group was "The Association for the Protection of Property Against Republicans and Levellers."

[20] Brown, *French Revolution,* p. 112.

[21] *New Annual Register,* 1794, pp. 190–1.

[22] For Thelwall, see C. Cestre, *John Thelwall, A Pioneer of Democracy in England* (London, 1906); B. S. Allen, "William Godwin's Influence upon John Thelwall" in *P.M.L.A.* 37 (1932); and Thompson, *Making of the English Working Class,* esp. pp. 157–161.

[23] Thompson, p. 160.

> Moderation! Moderation! A compromise between right and wrong! I detest it . . . for what is moderation of principle, but a compromise between right and wrong; an attempt to find out some path of expediency, without going to the first principles of justice. Such attempts must always be delusive to the individual and fatal to mankind. If there is anything sacred, it is principle! Let every man investigate seriously and solemnly the truth and propriety of the principles he adopts, but having adopted, let him pursue them into practice: let him tread on the path which they dictate.[25]

The change in direction Thelwall brought to the once sober L.C.S. did not go unchallenged within the organization, and within its own meetings, quietly and beneath the surface there occurred the first skirmish between his tactics and Godwin's principles. A serious split developed in the society over the issue of large mass meetings, a split between Thelwall's activist followers and the disciples of Godwin who saw the group as merely engaged in educational work disseminating political wisdom and truth through publication and discussion. The Godwinians lost, as evidenced by the resignation of twenty-one members, "who deplored the increase of factious spirit, the preference given to measures the most inconsiderate and violent." The General Committee of the society answered with an attack on "those many weak minded persons who have attempted to propagate an opinion, that all endeavors to promote the cause of liberty in Britain by means of popular associations must necessarily prove fruitless." Under Thelwall's leadership, Place informs us, many in the society came to believe that repeated mass meetings and agitation would provoke a crisis that would "force the House of Commons to consent to radical reform in the state of representation."[26] Place, a good Godwinian, resigned, however, rather than agree to such coercive tactics.

This disagreement took place solely within the radical family, but the next and more devastating confrontation took place out of doors for all to see. It came to a head in late 1795 and early 1796 with the introduction into Parliament of Pitt's notorious Anti-Sedition Acts. Pitt's response to L.C.S. agitation was not the reform of Parliament but instead the introduction of two laws abrogating freedom of speech, freedom of assembly, and freedom of the press. Fearful for its survival, Pitt's Government was lashing back. Food riots had spread through England in the summer and autumn of 1795, inflamatory tracts appeared at the same time with titles like *King Killing, the Reign of the English Robespierre,* or the *Happy Reign of George the Last.* En route to Parliament, the King was hissed and booed by some two hundred thousand Londoners thronging the streets. His carriage window was broken by a stone, and he is alleged to have gasped, "My Lord, I, I, I've been shot at." For many, the nightmare world of Versailles seemed to have arrived at St. James.[27] The Anti-Sedition Acts were Pitt's response to all of this. The Foxite opposition fought the bills unsuccessfully, and upon their passage *habeas corpus* was suspended for eight years. The two acts having been passed, Pitt's repression succeeded. The L.C.S. and the English Jacobins were stilled, only to revive in the short-lived naval mutinies of 1797.

Where did Godwin, the anarchist and philosopher of freedom and truth, stand in all of this? As a careful reading of *Political Justice* might have predicted, he was defending Pitt and attacking the L.C.S. and most specifically his one time friend, John Thelwall. His position is found in his pamphlet, *Considerations on Lord Grenville's and Mr. Pitt's Bills Concerning Treasonable and Seditious Practices and Unlawful Assemblies,* published anonymously by "a lover of order." (Did he find it so hard to betray a friend that he violated his own canon of candor?) The pamphlet begins with a general lecture on the principle of philosophical anarchy, as differentiated from false anarchy, offered by one "untainted with the headlong rage of faction." Governing men in a "petty and limited circle," he contends, is easy and can be done not by institutions but by reason alone. All men will "exercise an inspection over all," and no deeds can be concealed from the general censure or applause. But in nations with millions of men, "there is no eye penetrating enough to detect every mischief in its commencement." Governments are needed in such states to keep order and provide security. Shifting to the language of Burke, Godwin describes the fragility and complexity of society and government.

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[24] Thompson, p. 141ff.

[25] *The Tribune,* No IX, Saturday, 9 May 1795. Thelwall's weekly newspaper was reprinted in collected volumes in 1796. (Vol. I, 214).

[26] Add. Mss. 27, 815 f/165–6; f/184. (Letters of L.C.S. 1795–6). Add. Mss. 35, 143 f/15.

[27] (Anon.) *Truth and Treason! or a Narrative of the Royal Procession* (London, 1795); See also J. H. Rose, *William Pitt and the Great War* (London, G. Bell and Sons, 1911) pp. 282–88; and Thompson, pp. 141–143.

> He that deliberately views the machine of human society, will even in his speculations approach it with awe. He will recollect with alarm, that in this scene "fools rush in, where angels fear to tread." The fabric that we contemplate is sort of a fairy edifice, and though it consist of innumerable parts, and hide its head among the clouds, the hand of a child almost, if suffered with neglect, may shake it into ruins.[28]

There are, to be sure, abuses in societies; there are, for example, large numbers of poor and very few rich. But these abuses, Godwin quickly adds, are woven into the fabric of society and must be painstakingly corrected with careful judgment and deliberation lest one destroy faster than one creates, and rear up only false anarchy and barbarism. "These universal principles of political science," Godwin writes, "are necessary as premises" to the examination of Pitt's legislation. They convince one that government has as its major responsibility the protection of order and security, "the blessings we already possess, from the rashness of presumptuous experiment." The major threat to this security, Godwin insists, is the L.C.S. Like the Jacobin party in France, it is filled with impetuous and ardent zealots. It threatens civil order by "the immense multitudes it collects" who wreak disorder in the city streets. Its speeches and resolutions lack all temperance. Having done with the L.C.S., Godwin turns next on its leader Thelwall, whose lectures and public addresses violate all reasonable tenets of political life. The public mind ought to be enlightened, Godwin insists, and a uniformity of understanding reached such that no minister or monopolist would ever be powerful enough to withstand it. Could such a transformation occur in Thelwall's crowded, noisy meetings? No, he answers, and he condemns Thelwall:

> Oh reform! Genial and benignant power! How often has thy name been polluted by profane and unhallowed lips! How often has thy standard been unfurled by demagogues, and by assassins been drenched and disfigured with human gore![29]

Thelwall, Godwin continues, is a dangerous demagogue. "An impatient and headlong reformer," he is in no position to weigh his words with proper deliberation and purvey the truth while prompted by the demoralizing stimulus of the clamorous applause of large, excited audiences. He appeals to their passions and not reason, and if as "saving clauses," he urges the practice of universal benevolence and utters warnings against violence, he is like "Lord George Gordon preaching peace to the rioters in Westminster Hall," or "Iago adjuring Othello not to dishonor himself by giving harbour to a thought of jealousy."[30]

A shocked Thelwall lamented in his biweekly issues of the *Tribune* that even Godwin, his friend for so long, should have turned on him when all the forces of Pitt's repression sought to shut him up.[31] But Thelwall should have known that candor and sincerity were more important values for Godwin than friendship and that true to his own teaching, Godwin was obliged to censor his neighbor, to show his shortcomings, and thus to contribute to the formation of a more virtuous character.

The Elite and the Masses: Godwin the Anarchist—Some Concluding Comments

What most alarmed Godwin about the L.C.S. was the absence at its meetings of "persons of eminence, distinction, and importance in the country," who could temper the enthusiasm of those "not much in the habits of regular thinking." In addition, Godwin charged that Thelwall, the popular leader, was "not calmed and consecrated by the mild spirit of philosophy."[32] As in his attack on reformers in general, the intellectual and elitist disdain shown here was by no means a novel departure in Godwin's public writings. *Political Justice* had been just as outspoken on this score. There, for example, Godwin described the greater part of people as "mere parrots" who mouthed arguments of which they understood little or nothing. Burke's depiction of the democratic masses constantly setting their minds on every "floating fashion and fancy," was no more arrogant than Godwin's castigating a constantly changing public opinion by writing "... what is it they desire? They know not. It would probably be easy to show that what they professed to desire is little better than what they hate" (I, 255). One obvious answer, then, is given to the question raised at the end of Part II in this paper. "Political simplicity" cannot be realized by popular agitation led by radical reformers. Nor, for that matter, would widespread popular education suffice. Unlike Tom Paine and Mary Wollstonecraft, both of whom advocated a government-run school system, Godwin saw schemes of national education as only further examples of coercive interference with the individual's unfettered exercise of private judgment (II, 297–304).[33]

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[28] Godwin *Considerations* . . . (London, 1796) p. 4; 2–3.

[29] *Considerations* . . . the reader will have noted in this paper the incredible number of parallels found in these events with those in contemporary America, including, as we shall see, the tension between radical intellectuals and radical street (movement) politicians.

[30] *Considerations* . . . p. 22.

[31] *The Tribune*, Vol. II, vii–xiv; Vol. III, 101–103.

[32] *Considerations* . . . pp. 17, 19, 20.

If the age of felicity could not be entered through the schoolhouse, nor through street pressure on a weak Parliament, how would it be accomplished? How would the simple and sincere social order come about? Enlightened despotism, according to Godwin, was clearly not the answer. Power and force could never be the allies of truth and reason; nor for that matter could monarchs ever be capable of encouraging progress. The same could be said for the clergy and aristocracy. As for the middle class of newly enriched commoners, they too were ruled out as too selfish to be champions of reform. The lawyers who favored the rich over the poor and discouraged candor were also incapable of this task (II, 50–90, I, 41; 18–19; II, 457ff.; II, 390; 423; 407). The only social group capable of the noble mission of moving society to its blissful future was for Godwin, as for legions of future anarchists, the literary and intellectual elite. At bottom, Godwin harbors the one basic prejudice shared by all the enlightenment philosophers, save Rousseau—the unreflecting faith in the power of reflective man. *"Political Justice,"* he informs the reader in his preface, is "an appeal to men of study and reflection." If only a small number of "liberally educated and reflecting members" would serve as the "people's guides and instructors," then, Godwin holds, "the business would be done." The "elevated system of the preceptor" would push aside "the groveling views of the great mass of mankind." These few preceptors "having stored their minds with reading and reflection" would communicate their wisdom to all, and through *them,* not through national schooling, enlightenment would filter down to all mankind (I, xi; 104; 50; 295–6). The social and political mission of the intellectual elite is most succinctly described by Godwin in his *Enquirer* of 1797.

> Men of genius must rise up . . . to analyze the machine of society, to demonstrate how the parts are connected together, . . . and point out the defects and the remedy. It is thus only that important reforms can be produced. . . . He who is a friend to general happiness, will neglect no chance of producing in his pupil or his child, one of the long-looked for saviours of the human race.[34]

There is no doubt who these men of genius were. They could be found in the literary and philosophical circle that always had Godwin at its center, the circle of Holcroft and Godwin's wife, Mary Wollstonecraft, and that of Coleridge, Hazlitt, and Godwin's son-in-law, Shelley. Writing to a friend, Godwin unabashedly described himself and his correspondent as "of course among the few enlightened." His close friend Hazlitt wrote that Godwin "has the happiness to think an author the greatest character in the world."[35] His friend Coleridge, however, soon to leave the fold of eighteenth-century rationalism, did not even in 1795 share its and Godwin's high hopes for the intellectual elite. In two sentences he repudiates the intellectualism of Godwin in particular and the *philosophes* in general.

> The annals of the French Revolution prove that the knowledge of the few cannot counteract the ignorance of the many . . . the light of philosophy, when it is confined to a small minority, points out the possessors as the victims rather than the illuminators of the multitude.[36]

In his writings and in his actions Godwin exemplifies the characteristic intellectualism of the Enlightenment. How fitting, then, that in 1795 he should have locked horns with the democrat Thelwall, for in this obscure confrontation an important turning point is marked in the evolution of modern political attitudes. The philosophical and theoretical radicalism of the eighteenth century, the work largely of speculative intellectuals colored by elitist assumptions, meets head on the practical radicalism of the Nineteenth century, the work mostly of political agitators and reformers with democratic popular sympathies. The days would soon be gone when radicals would write, as Godwin did, that men of character would emerge in times of crises and the "soil in which such men are to be matured, is less that of action, than of enquiry and instruction" (I, 298). Interestingly enough, the symbolic aspects of this transition were captured best by Thelwall, the victim of Godwin's intellectual snobbery. To Godwin's charge that in the politics of agitation and reform, one "bids farewell to the patient lubrications of the philosopher, and the labor of the midnight oil" (II, 368), Thelwall responded through his *Tribune* that, on the contrary:

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[33] For Paine's views see *Rights of Man,* Part II. (London, Penguin Books, 1970). For Wollstonecraft's views see *The Vindication of the Rights of Woman* (London, 1792), p. 185. See also the useful book *Education and Enlightenment in the Works of William Godwin* by B. R. Polin (New York, 1962), especially Ch. IV.

[34] *The Enquirer,* pp. 10–11.

[35] William Hazlitt, *The Spirit of the Age* (London, J. M. Dent & Sons, 1964), p. 202; C. Kegan-Paul, *William Godwin: His Friends & Contemporaries* (London, 1876), II, 195.

[36] S. T. Coleridge, *Essays on His Times* (London, 1850), I, 7–8.

> It is in the mixed and crowded audiences in theaters and halls of assembly that the real lover of his species must principally expect to inspire that generous sympathy, that social ardor without which a nation is but a populous wilderness and the philosopher himself only a walking index of obsolete laws and dead-letter institutes.[37]

This division within the Jacobin camp in 1795 sets the pattern for the evolution of nineteenth-century radicalism and has its counterpart even to this very day. Most friends of humanity react to the ills of society with a political response and like Thelwall seek political goals through political action. A smaller number, however, still cling to Godwin's philosophical response and flee from politics to the vision of a nonpolitical society achieved through the nonpolitical means of education and enlightenment.

One should also note the significance of this split for the general development of anarchist thought, which is where this discussion of Godwin began. Many anarchists would leave Godwin the *philosophe* behind. Much of nineteenth- and twentieth-century anarchism is, of course, an outright repudiation of the Enlightenment rationalism that the founder of anarchism so typified. Ineed, for many more recent anarchists, politics is in fact equated with reason and because of this to be superseded with and by apolitical action. In the same vein, it could be argued that Thelwall's street politics was in fact a more accurate anticipation of the future anarchist style. But, these points notwithstanding, Godwin stands as the father of modern anarchist thought in a sense more profound than simply being the first to write of society without government.

He gives to anarchism qualities that endure despite the philosophical, methodological, and cultural gap that may separate him and the doctrine's later proponents. As it was in his presentation, the doctrine after him is forever torn between the liberal values of individuality, independence, autonomy, privacy, and self-determination, on the one hand, and the nonliberal values of community, solidarity, and the encouragement of virtue through social pressure, on the other. So, too, later anarchists waver, like Godwin, between a progressive, futuristic orientation with assumptions of perfectibility and endless innovation and improvement, and a nostalgic yearning for a simple, agrarian, and preindustrial existence. Most decisively, however, he has stamped anarchism with its elitism, its abiding convictions that if only all men were as wise or as sensitive as the anarchist then governments would be superfluous, and that until this was the case government represented pure coercion. The artist-anarchist's role meanwhile is to emancipate the ignorant "great mass of mankind," the misguided "Pig Nation," whether slowly and imperceptibly through reason and enlightenment or hastily with the dramatic flourish of an exquisite act. Finally, Godwin in these years may well have marked anarchism with another indelible stamp, its ultimate service to the status quo (although latter-day anarchists may have been and are less willing in this service than he). To the extent that change in political systems—either incremental or revolutionary—has and must come from political activity, there is little to fear from those who seek change but who eschew politics, be it for philosophic enlightenment or for theater.

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[37] *The Tribune*, II, xiv.

Congress and the Defense Budget: 1960–1970*

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Introduction

Increasing dissatisfaction with our defense policies and growing demands to reorder our national priorities have led to renewed interest in the role of Congress in the shaping of national security policy. What some critics regard as the unresponsiveness of the executive has stimulated hopes that the Congress might play a greater part in directing our defense effort. The conclusion which emerges from a survey of the scholarly literature, however, is that Congress has performed neither an active nor a policy-oriented role in the past and that the prospects for the future are likewise dim. Senator Proxmire's description of Congress as a "pushover for the Pentagon"[1] echoes the conclusions of scholars who have found that Congress does not address the strategic issues nor debate national security objectives.[2]

Congressional influence in the shaping of defense policies could assume at least two forms: (1) Congress simply could make nontrivial dollar changes in the President's budget request, and (2) these budget changes might be attributable to Congressional policy concerns and program preferences rather than to other motives, especially a desire simply to reduce the level of defense spending. The first condition relates to the *fiscal impact* of Congressional power over defense appropriations. The second condition relates to Congressional *motives* for making changes in the President's budget.

Conceptually, the two elements are largely independent of each other. Congress could have a substantial impact on defense spending by imposing large across-the-board reductions. This would have important but largely unpredictable consequences for policy. Alternatively, Congress might make selective changes in the budget in order to encourage or to inhibit particular defense policies. These changes would not necessarily be reflected in large dollar changes in the total budget.

Whoever seeks to increase Congressional involvement in defense policy must be cognizant of both aspects. But Congressional motivation is the higher hurdle for those who hope for increased Congressional participation. If Congress already views its control over appropriations as a means by which to influence defense strategy and programs, the goal of Congressional activism in national security policy is more than half realized: Congress's image of its function is already established, and it need only be persuaded to become more involved or to pursue alternative objectives.

A conventional perspective on the last decade might suggest that Congress has been deficient on both counts, that it has sought to serve fiscal motives but has achieved only a small fiscal impact. Studies of the budgetary process which concentrate on nondefense spending conclude that the Congress's orientation to appropriations is essentially "non-programmatic"[3] and lead to the inference that Congress is disinclined to use its power over the defense budget as an instrument to shape programs and policies. Generalizing from these studies, we might expect that Congressional changes in the President's defense budget stemmed primarily from *fiscal* motivations, i.e., the desire to save money *per se*, to eliminate waste and inefficiencies.[4] From

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* This study was undertaken while the author was a Research Fellow in Foreign Policy Studies at the Brookings Institution. The advice and comments of John Ferejohn, Leslie H. Gelb, Morton Halperin, Kerry L. Jones, Craig Liske, and John Manley are gratefully acknowledged.

[1] Senator William Proxmire, *Report From Wasteland: America's Military Industrial Complex.* (New York: Praeger, 1970), p. 122.

[2] The literature is surprisingly silent on this aspect of Congressional behavior during the 1960s, although it is a fairly common description of Congress during the preceding decade. See, for example, Raymon H. Dawson, "Congressional Innovation and Intervention in Defense Policy," *American Political Science Review*, 56 (March, 1962), 44; and Lewis Anthony Dexter, "Congressmen and the Making of Military Policy," in Robert L. Peabody and Nelson W. Polsby, editors, *New Perspectives on the House of Representatives* (Chicago: Rand McNally, 1963), p. 306. For a contrary view of the same period, see Samuel P. Huntington, *The Soldier and the State* (Cambridge: Harvard University Press, 1959), p. 409; ". . . Congress . . . gives considered and effective attention to the major issues of military policy involved in the budget."

[3] Aaron Wildavsky, *The Politics of the Budgetary Process* (Boston: Little, Brown, 1964), p. 136. Cf. Allen Shick, "Systems Politics and Systems Budgeting," *Public Administration Review*, 29 (March-April, 1969), 139.

[4] Cf. Morris Janowitz, *The Professional Soldier* (New York: The Free Press, 1960), p. 354. "Fiscal" motivations, as the term is used here, combines Goss's categories of "procedural" and "fiscal" Congressional influence. See Carol F. Goss, "Congress and Weapons Procurement," paper prepared for delivery at the Sixty-sixth Annual Meeting of the American

fiscal 1960 through fiscal 1970, however, Congressional changes in the President's budget request for defense rarely exceeded 2 per cent.

The conventional wisdom which holds that Congress has been unwilling to address the defense budget in policy terms tends to be based on a superficial reading of highly aggregated data.[5] An exploration within the defense budget, specifically, of the separate appropriation titles and individual budget line-items, suggests that Congress has had more impact in some areas than others, and, more importantly, that Congressional budget activity has been concerned with more than reducing the level of expenditures.

Both cross-sectional and longitudinal variations in Congressional budget activity support this view. Congressional changes in the President's defense budget have been concentrated in two areas—Procurement, and Research and Development—rather than equally distributed among all the line-items. Congressional activity across time has varied directly with the intensity of national debate about our defense policies. Neither fact convincingly disproves the notion that Congress's approach to the defense budget has been simple budget cutting: these may merely be the areas and times when the defense budget has been most accessible to fiscal concerns. These findings do, however, raise some doubts about the adequacy of the "nonprogrammatic" explanation, because they are precisely the results which would be expected if Congress did address the budget in policy terms and did use the power to appropriate as a tool to influence the shape of defense programs.

The purpose of this paper is twofold. First, it demonstrates that the distribution of Congress's changes in the President's defense budget is consistent with the hypothesis that Congress has a *programmatic* as well as a fiscal orientation toward defense spending; that is, Congressional judgments regarding specific defense programs and their relation to national security, rather than an exclusive concern with reducing spending and eliminating waste, explain Congress's scrutiny of the defense budget. Second, it seeks to illuminate the roles and relative influence of the House and Senate in the defense appropriations process and to highlight some of the problems of interpretation which are relevant to the broader study of the budgetary process.

The Data Base

The study examined four of the stages through which the defense appropriations[6] pass every year: the President's budget request to the Congress, House *floor* action, Senate *floor* action, and the conference report.[7] Four of the five appropriations titles were included: Personnel; Operations and Maintenance (O&M); Procurement; and Research, Development, Testing, and Evaluation (RDT&E). Military construction, which is dealt with separately in Congress, was not included. Twenty-eight individual line-items included within these four appropriation titles were selected for study.[8] Since the budget format has been more or less stable since fiscal 1960, the eleven years from fiscal 1960 through fiscal 1970 define the period of the study. Budget *amendments* were included in the data; budget *supplementals* were excluded.

Before analyzing these data, one should note that aggregating the defense budget by line-item presents important drawbacks for a study of Congressional activity regarding specific national security programs. This is because the funds for particular programs may be distributed among several line-items, and each line-item may include money for several programs. Thus, funds which support the fleet of Polaris submarines may be included under the line-item for "Navy Shipbuilding and Conversion" (Procurement), as well as "Navy O&M," while the single line-item "Navy Shipbuilding and Conversion" covers the procurement of de-

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Political Science Association, Los Angeles, September 8–12, 1970. It should be noted that another source of Congressional activity—the desire to advance a political career—is not addressed in this paper.

[5] For a more general discussion of shortcomings of aggregate data, see Joseph La Palombara, "Macrotheories and Micropolitics in Comparative Politics: A Widening Chasm," *Comparative Politics*, 1 (October, 1968), 52–78.

[6] The exclusion from this study of defense *authorizations* seemed permissible primarily because, for the vast majority of the cases considered, neither language nor dollar amounts of authorization acts limited the appropriations actions. Cf. Jack D. Fleer, "Congressional Committees and the Making of Foreign Policy: Authorizations and Appropriations for Major Weapons Systems in the Legislative Process," unpublished Ph.D. Dissertation, University of North Carolina, 1964; and Herbert W. Stephens. "The Role of a Legislative Committee in the Appropriations Process: A Study Focused on the House Armed Services Committee," unpublished Ph.D. Dissertation, Florida State University, 1967.

[7] Although the data in this study are based on floor action, appropriations bills reported out of committee very rarely are changed on the floor. Neither the relationship between the appropriations committees and their parent bodies nor the opportunity for influence inherent in that relationship is addressed in this study.

[8] See Appendix for a list of these line items.

stroyers and aircraft carriers as well as submarines.

These line-item data are employed in this analysis because they represent the only form of disaggregation of the total defense budget which is publicly available and the form in which Congress considers the defense budget and reports its appropriations actions. (Congress also receives detailed information regarding specific weapons systems and projects, but these data cannot be combined to yield the total budget.) Moreover, the different appropriations titles roughly describe the path which weapons systems follow from drawing board to field deployment. New weapons are conceived, designed, developed and tested with funds budgeted under RDT&E. They are produced in quantity with funds from the Procurement budget. The manning and servicing of weapons in the inventory are paid for with funds from the O&M and Personnel budgets.

The Dollar Impact of Congressional Budget Activity

The dollar magnitudes of Congressional changes in the President's budget requests for defense have not been large. Table 1 indicates the relative scale of Congressional activity. From fiscal 1960 through fiscal 1970, five annual appropriations involved changes of less than 1 per cent, seven of less than 2 per cent, and nine appropriations out of eleven varied from the President's request by less than 4 per cent. As one standard of comparison, Richard Fenno considered changes of less than 5 per cent to be "marginal" in his survey of nonmilitary appropriations from 1947 to 1962.[9] Thus, when compared to changes in the nondefense budget, Congressional alterations in the President's budget requests for defense have been small.

Congressional budget changes also have been much smaller than the reductions made by the administration in the budget estimates submitted by the military services. Although Congressional changes rarely exceeded 4 per cent, the administration regularly reduced the services' estimates by from 10 to 20 per cent.

When compared either to Congressional changes in the nondefense budget or to Presidential reductions in the services' budgets, the dollar impact of Congressional changes in the defense budget has been modest. Congress's power to appropriate funds did not yield large changes in the levels of defense spending during the 1960s. Other dimensions of Congressional activity should be evaluated within this context.

Table 1. Percentage Change in Budget Requests

Fiscal Year	President's Change in Services' Budget Requests	Congressional Change in President's Budget Requests
	(%)	(%)
1960	−19.98	−0.06
1961	−10.42	+1.68
1962	− 8.07	+0.58
1963	− 9.78	+0.48
1964	−19.70	−3.66
1965	−28.09	−1.51
1966	−20.10	−0.18
1967	−21.59	+0.70
1968	−22.91	−2.30
1969	−24.09	−1.75
1970	−24.69	−7.49

Source: Department of Defense, Office of the Assistant Secretary of Defense (Comptroller).

The dollar magnitude of Congressional changes in level of *total* defense spending obscures variations which occur within the budget. Table 2 shows Congressional changes in the defense budget by each appropriation title. Congress almost always has made larger changes in Procurement and RDT&E than in the other two budget areas. As a result, computation of budget changes based on the total defense budget consistently understates the size of the change in the former two appropriations titles and exaggerates the changes in Personnel and O&M. While the dollar magnitude of Congressional changes has been small, it has been somewhat greater for those budget categories which contain funds for the development and production of weapons systems.

The Focus of Congressional Attention

Two measures of Congressional activity were employed: (1) the presence or absence of changes in the President's budget request, by line-item, and (2) the size or frequency of Congressional changes in the line-items. Both measures indicate that Congressional activity has been concentrated in particular budget categories.

Table 3 classifies the 308 budget outcomes under consideration according to whether the conference report was equal to either the House or Senate, equal to neither ("Compro-

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[9] Richard F. Fenno, Jr., *The Power of the Purse: Appropriations Politics in Congress* (Boston: Little, Brown, 1966), p. 353. Cf. Bruce M. Russett, *What Price Vigilance?* (New Haven: Yale University Press, 1970), pp. 26, 187; and Proxmire, *Report from Wasteland* . . . pp. 91, 110–11.

TABLE 2. Congressional Changes in President's Budget Requests, by Appropriation Title

	President's Budget Request (in Millions)	Congressional Change (%)
FY1960		
Personnel	11,625	+0.11
O&M	10,512	−0.65
Procurement	13,348	−0.09
RDT&E	3,772	+1.19
Total	39,257	−0.06
FY1961		
Personnel	11,813	+0.18
O&M	10,527	−0.59
Procurement	13,085	+3.32
RDT&E	3,910	+6.85
Total	39,335	+1.68
FY1962		
Personnel	12,746	−0.46
O&M	11,793	−0.53
Procurement	16,860	−1.10
RDT&E	4,790	+9.48
Total	46,189	+0.58
FY1963		
Personnel	13,050	−1.12
O&M	11,569	−0.03
Procurement	16,445	+1.23
RDT&E	6,843	+2.62
Total	47,907	+0.48
FY1964		
Personnel	13,235	−2.90
O&M	11,792	−0.66
Procurement	16,725	−6.09
RDT&E	7,262	−4.31
Total	49,014	−3.66
FY1965		
Personnel	14,597	−0.20
O&M	12,396	−0.67
Procurement	13,756	−2.43
RDT&E	6,722	−4.06
Total	47,471	−1.51
FY1966		
Personnel	14,560	+0.27
O&M	12,472	+0.17
Procurement	11,412	−0.07
RDT&E	6,709	−2.07
Total	46,852	−0.18
FY1967		
Personnel	18,676	+0.29
O&M	15,675	+0.18
Procurement	16,408	+1.43
RDT&E	6,905	+1.14
Total	57,664	+0.70
FY1968		
Personnel	22,001	−1.00
O&M	19,136	−1.46
Procurement	22,917	−4.00
RDT&E	7,273	−2.25
Total	71,584	−2.30
FY1969		
Personnel	23,014	−1.92
O&M	22,787	−4.49
Procurement	23,254	−14.07
RDT&E	8,006	−5.68
Total	77,074	−6.75
FY1970		
Personnel	24,377	−3.31
O&M	21,792	−4.28
Procurement	20,887	−14.58
RDT&E	8,222	−10.37
Total	75,278	−7.49

Source: Department of Defense, Office of the Assistant Secretary of Defense (Comptroller).

mise"), equal to both the House and Senate but not equal to the President's request ("Same"), or equal to the President's request in all three Congressional budget stages ("No Change"). In 22 per cent of the budget outcomes, neither house of Congress made any change (either increase or decrease) in the President's request; about a quarter of the President's budget requests were, if not unexamined by Congress, at least untouched by either house.

Table 4 suggests that Congressional inaction has been concentrated[10] in the Personnel cate-

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[10] Since each appropriation title includes a different number of line-items, a measure of relative concentration of "No Change" outcomes (Table 4) is obtained by taking (1) the incidence of "No Changes" in Personnel as a percentage of the total number of "No Change" outcomes, (2) the number of Personnel budget decisions as a percentage of the total number of budget decisions, and (3) dividing the former by the latter. If the incidence of "No Change" outcomes were distributed in the same proportion as the total line-items, the relative concentration would equal 1.00. Index values greater than 1.00 indicate a greater than expected incidence of "No Change," and values less than 1.00 indicate a fewer than expected number of cases. Thus, 13 per cent of "No Change" outcomes occurred in Procurement and only 6 per cent in RDT&E but, owing to the greater number of line-items included under Procurement, the relative concentration of "No Change" indicated in Table 4 is about equal for both categories (.45 vs. .43).

Table 3. Distribution of Appropriation Outcomes, by Appropriation Title, FY1960–FY1970

Appropriation Outcome	Personnel	O&M	Procurement	RDT&E	Total
	(%)	(%)	(%)	(%)	(%)
House	05	08	07	05	06
Senate	26	18	17	14	20
Compromise	02	38	42	43	27
Same	29	17	24	30	25
No Change	38	20	10	09	22
Total	100% (110)	100% (66)	100% (88)	100% (44)	100% (308)

gory. If Congressional activity were equally distributed among the appropriation line-items, the index of relative concentration would equal 1.00 for all categories. The high concentration (1.72) of "No Change" outcomes in the Personnel category indicates that the President's budget requests for these items were quite likely to be left untouched by Congress. By contrast, the low concentration of "No Change" outcomes in Procurement (0.45) and RDT&E (O.43) shows that the President's requests in these areas were very likely to be changed by Congress. Congressional attention and activity have been concentrated in the latter categories at the expense of the former.[11] The concentration of "No Change" outcomes (0.90) was also relatively high in the O&M category. Although the evidence is not nearly as strong as the case of Personnel, it nevertheless seems reasonable to suggest that budget decisions relating to Operations and Maintenance have commanded relatively little Congressional attention.

The Personnel category constitutes about 30 per cent of annual Department of Defense expenditures and O&M contributes 27 per cent.(Procurement represents about 32 per cent and RDT&E the remaining 11 per cent.) Thus, more than half the budget seems to receive a relatively low level of Congressional attention, as measured by the likelihood of changing the President's budget requests. This helps to explain why the total DOD budget has shown such small Congressional changes in the executive budget, viz., even relatively large changes in Procurement and RDT&E are likely to be blunted and obscured by that half of the total budget which changes hardly at all.

Table 4. Relative Concentration of "No Change" Outcomes, FY 1960–FY 1970

Personnel	O&M	Procurement	RDT&E
1.72	.90	.45	.43

<1.00=less than expected concentration.
>1.00=greater than expected concentration.

Thus far, our measures of Congressional activity have been restricted to the most extreme cases—the presence or absence of Congressional changes in the President's budget. A more complete picture may be obtained by a measure which is able to distinguish budget categories in which there were small or infrequent changes in the President's budget from those in which there were large or frequent changes.

The absolute value of the difference between the President's budget request and the Congressional appropriations (computed as a percentage of the President's request) is one measure of the distribution of Congressional activity. The mean annual absolute difference between the President's request and Congress's appropriations supplies a useful summary index of Congressional activity for the period under consideration. If Congress made no changes in a particular budget category in the eleven year period, the index of activity = 0.00. Large or frequent Congressional changes in the President's requests (either increases or decreases) will yield larger values for the index.

The level of Congressional changes for each of the four budget categories and for the total DOD budget (included as a benchmark) is reported in Table 5. These results confirm the observations based on the distribution of "No Change" outcomes reported above. The mean annual absolute difference for the total defense budget is 2.308. The Personnel budget category has the smallest values (1.069), indicating minimal Congressional activity. Procurement and RDT&E rank highest (4.401 and 4.547 respectively), suggesting that Congressional attention and activity have been focused on matters relating to the procurement of weapons systems. O&M (1.246) is closer to Personnel than to either of the other two appropriation titles.

One may conclude from these results that Congressional activity, and inferentially, Congressional interest, is concentrated on Procurement and RDT&E matters at the expense of Personnel and O&M. Congress does not ordinarily impose "across-the-board" cuts on the

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[11] If "No Change" is an indication of minimal Congressional interest, "Compromise" suggests the reverse case in which both the House and the Senate were sufficiently attentive to make changes in the President's request, and were sufficiently committed to their respective positions so that neither house yielded completely to the other in the conference.

Table 5. Congressional Percentage Changes in President's Budget Requests, Mean Annual Absolute Difference

	Personnel	O&M	Procurement	RDT&E	Total DOD
Mean Absolute Difference	1.069	1.246	4.401	4.547	2.308

defense budget. The total DOD budget appears to emerge from the Congressional budget stages virtually unscathed largely because more than half of that total is virtually untouched as it passes through Congress.

Congressional Activity: Fiscal or Programmatic?

Given the size and complexity of the defense budget, and the limited resources available to the Congress to monitor it, it is not surprising that Congress has chosen to focus its resources on selected areas rather than risk dissipating its energies by surveying the entire budget in equal, if not great, detail.[12] The question remains, however, why the Congress appears to have consistently concentrated on Procurement and RDT & E at the expense of Personnel and O&M. The following speculations are offered to guide to further research on the question.

Wildavsky and Fenno in their research on the nondefense budget have suggested that Congress is inclined to emphasize the fiscal, or dollar-level, aspects of the budget at the expense of a program orientation.[13] Congress, it is implied, is less concerned with what is obtained than with what it will cost. It does not seem likely, however, that Congress is focusing its attention on Procurement and RDT & E merely in order to maximize the short-term economic impact of defense expenditures; a purely fiscal orientation would lead it to other areas of the budget.

In order to evaluate the expected short-term fiscal impact of Congressional appropriations, it is necessary to distinguish between those *appropriations* and the *expenditures* which are allowed by them. Congress appropriates in terms of "new obligational authority" (NOA)—permission to incur additional financial liabilities up to the limit set by the appropriation. Expenditures permitted by that NOA need not occur in the same year in which funds are appropriated. Nearly all funds budgeted for Personnel are expended in the same year in which they are appropriated. By contrast, only about sixty cents of every Procurement dollar is spent in the same year as obligated, the balance being expended in succeeding years.[14] Thus, if Congress were seeking to have an impact on the level of defense *expenditures* in the next fiscal year, it would be adjusting the Personnel budget rather than the Procurement budget. In fact, if influencing the short-term level of federal expenditures by changing the defense budget were a serious objective of Congress, the distribution of its activity would be almost precisely the *opposite* of that which is observed.[15] Congress has evidenced concern about both the price and the *product*.

Several factors direct Congressional attention to Procurement and RDT&E. Fenno and Wildavsky suggest that Congress seeks to compensate for its limited resources for coping with the size and complexity of executive budget requests by focusing primarily on *changes* made in the previous year's budget and *changes* proposed in the present year's request.[16] The Procurement and RDT&E categories typically contain the largest number of recent and proposed program changes. On the basis of studies of the nondefense budget, we would expect Congressional scrutiny to be directed to these budget categories.

Congress's alleged inclination to take incremental, remedial, undramatic budget actions may also be a factor in directing its attention to Procurement and Research and Development. These budget categories can be disaggregated into individual programs with relative ease. Congress may make decisions about, and changes in, these individual weapons systems, one by one, with a sense that the decisions are more or less independent of one another.[17]

It must be emphasized, however, that Congressional activity cannot be explained simply as responses to year-to-year changes made by the President in the executive budget. A model of "simple incrementalism" might predict that changes in the line-items in the President's budget at time *t* (as compared with appropriations at time *t*-1) would stimulate Congressional changes in those line-items during the Congressional phase of the appropriations process.

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[12] See, for example, Fenno, *Power of the Purse,* p. 332.

[13] Wildavsky, *Politics of the Budgetary Process,* p. 60, Fenno, p. 316.

[14] Cf. *Congressional Quarterly Almanac,* 20, 1964, 154–55.

[15] This is supported by the experience of Projects 693 and 703, Pentagon budget drills to hold expenditures within Congressional and Presidential budget ceilings. In both cases, the largest reductions occurred in Personnel and O&M. I am indebted to Charles Shirkey and Arnold Kuzmack for this information as well as other assistance.

[16] Fenno, *Power of the Purse,* pp. 318, 333; and Wildavsky, *Politics of the Budgetary Process,* p. 59.

[17] Cf. David H. Davis, "The Price of Power," *Public Policy,* 18 (Spring, 1970), 377. Jernberg has found that certain committees' line of questioning indicates a programmatic orientation. See James E. Jernberg, "Information Change and Congressional Behavior," *Journal of Politics,* 31 (1969), 722–40.

The "simple incrementalism" model is suggested by the following description drawn from Wildavsky's study of the budgetary process:

> Budgeting is *incremental, not comprehensive* . . . [T]he men who make the budget are concerned with relatively small increments to an existing base. . . . As Representative Norrel declared in testifying before the House Rules Committee, "If *you will read the hearings of the subcommittees* you will find that most of our time is spent in talking about the changes in the bill which we will have next year from the one we had this year, the reductions made, and the increases made. That *which is not changed has very little, if anything,* said about it."[18]

According to this model, the previous year's appropriations constitute a budgetary "base," and budgetary review is devoted primarily to deviations (increases or decreases) from that base. The "simple incrementalism" model assumes that maintaining stability in funding levels is a primary Congressional objective, and to that end, Congress will seek to minimize deviations from the budgetary base.

This line of argument permits either of two conclusions. First, deviations from the base may merely attract Congressional attention and stimulate Congressional activity (whatever the motivation of the activity). That is, the locus of Congressional activity is largely dependent upon Presidential initiative. If this were the case, between-year changes in the President's budget would be expected to be highly correlated with within-year changes by Congress.

A second and obviously related conclusion is that Congress might be motivated by the objective of maintaining stable levels of funding by minimizing deviations from the budgetary base. If this were the case, annual changes in the President's budget would be negatively correlated with Congressional budget changes.

Neither prediction is supported by the data. There is virtually no relation between the size of the changes made by Congress in the President's budget line-items and year-to-year changes in the President's budget request ($r = -0.006$). Congress does not simply react to year-to-year changes in the executive budget. Its attention is attracted by features other than annual variations in the President's requests, and its objectives are not restricted to maintaining a stable level of funding.

The machinations of interested parties in the executive branch help to explain Congress's concentration on RDT&E and Procurement. The military services, particularly the Navy and the Air Force, have their greatest stake in these two areas. Both the claims for increased budget shares and the rhetoric of interservice rivalries (especially competition over roles and missions) are carried on in terms of present and future weapons systems. The civilians in the executive branch who are concerned with the defense budget are required to share the services' interest and similarly to concentrate their resources. Both the civilians and the military may be expected to transfer this focus of concern to the Congress as Congress acts on the budget decisions of the President. Appeals, requests, and demands for Congressional changes in the President's budget are most likely to be in terms of weapons systems, and therefore under the Procurement and RDT&E appropriation titles.

The importance of these categories to the executive is suggestive of another, and more fundamental, reason why Congress distributes its energies as it has. Just as struggles within the executive branch over the content and direction of defense policy are manifest as conflicts over weapons systems, Congress may believe that decisions relating to present and future weapons systems, reflected in Procurement and RDT&E budget actions, are at the very heart of national security policy, and constitute that part of the budget on which Congressional efforts to influence national security policy are most economically expended. As Charles Hitch, former Pentagon Comptroller, has observed: "These [weapons systems] choices have become . . . the key decisions around which much else of the defense program revolves."[19] If Congress were seeking to influence defense *policy* and, to that end, had a *programmatic* orientation toward budget decisions, the distribution of Congressional activity would parallel the actual behavior described above.[20]

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[18] Wildavsky, pp. 15, 59; and Otto A. Davis, M. A. H. Dempster, and Aaron Wildavsky, "A Theory of the Budgetary Process," *American Political Science Review*, 60 (September, 1966), 529–30. It should be noted that the "simple incrementalism" model, although related to the models evaluated by the latter authors, is not explicitly tested by them.

[19] Charles J. Hitch, *Decision-Making for Defense* (Berkeley: University of California Press, 1965), p. 23.

[20] Congress's interest in influencing the content of national security policy by means of its control of the pursestrings explains in part the reassertion by the Armed Services Committees of their right to

Table 6. Correlation(r) of Between-Year Budget Changes and Within-Year Budget Changes, FY 1960–FY 1970, by Appropriation Title

Personnel	−0.337
O&M	−0.212
Procurement	0.089
RDT&E	0.101

This suggests that the "simple incrementalism" model might better describe budget changes in those areas—Personnel and O&M—which provide less leverage for influencing defense policy. The relationship between annual changes in the President's budget and within-year Congressional changes should yield greater negative correlations for these appropriations titles than for Procurement and RDT&E.

The results reported in Table 6 lend some support to this hypothesis. Although none of the correlations is large, the rank order and signs are in the predicted direction. There is an inverse relationship between Presidential changes and Congressional alterations for the Personnel (r = −0.337) and O&M (r = −0.212) line-items as the "simple incrementalism" model would predict. The smaller and positive correlations for Procurement (r = 0.089) and RDT&E (r = 0.101) are contrary to the predictions of "simple incrementalism."

Nonprogrammatic factors such as focusing on changes and making incremental alterations which, according to the literature, account for Congressional attention to Procurement and RDT&E, may simply reinforce Congress's independent inclination to influence the direction and content of national security policy. As Fenno notes (regarding nonmilitary budgets):

> A program established last year or proposed for the coming year will receive careful scrutiny; and it is here that the *program preferences* of a key Committee member can work themselves most directly and decisively into legislative policy-making.[21]

An examination of what Dexter calls the "rhetoric of justification"[22] supports the conclusion that Congress has made changes in the Procurement and RDT&E budgets on the basis of its understanding of the requirements of national security. Congressional increases in the President's budget frequently are explained in these terms. In its report on the FY 1960 budget, the House Appropriations Committee took note of the need for limited war forces:

> We must . . . maintain a capability of coping quickly with any incidents which may develop as an additional assurance against a small affair growing into a much larger one.[23]

Believing that the Eisenhower administration had not adequately recognized the necessity of a strong limited war capability, the House increased the President's request for Army Procurement by 20 per cent.

The same House Report also noted that the mobility of forces-in-being was a more important contribution to the limited-war capability than was the ability to mobilize large reserves:

> It is not so much a matter of mobilizing immediately available manpower to handle a limited war as it is the capability of the necessary manpower to get quickly to the spot, adequately equipped and supported.[24]

In support of this strategic choice, the House created a new budget line-item, "Airlift Modernization," and appropriated more than $300 million for it in FY 1961. Similarly, the Senate Appropriations Committee exceeded the Administration request for a new manned bomber because it believed that ICBM's were not sufficiently reliable.

> We view the B-70 cutback and stretchout (planned by the Administration) with grave concern and serious reservations. . . . We question whether this nation can afford to place full dependence on an operationally untried and untested missile force . . .[25]

Budget reductions also have been explained in terms of the Congress's understanding of the requirements of national security. Senator Richard Russell articulated his uneasiness about procuring the fast deployment logistics (FDL) ships requested by the Johnson Admin-

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authorize on an annual basis the major portions of the Procurement and RDT&E budgets. See: Dawson, "Congressional Innovation . . . ," p. 43. For similar motivations influencing legislation regarding Pentagon reorganization plans, see John C. Ries, *The Management of Defense* (Baltimore: Johns Hopkins University Press, 1964), pp. 95–96, 104, and 141–142.

[21] Fenno, *Power of the Purse*, p. 318. For material on the Appropriations Committee's interest in policy see: Goss, "Congress and Weapons Procurement," p. 7; Dexter, "Congressmen and the Making of Military Policy," pp. 316–17; and Bernard K. Gordon, "The Military Budget: Congressional Phase," *Journal of Politics*, 23 (November, 1961), 693–94.

[22] Dexter, p. 306.

[23] U.S. House of Representatives, Committee on Appropriations. *Report on Department of Defense Appropriations Bill for Fiscal Year 1960.* 86th Congress, 1st Session, Report No. 408, p. 7.

[24] U.S. House of Representatives, Committee on Appropriations. *Report on Department of Defense Appropriations Bill for Fiscal Year 1960.* 86th Congress, 1st Session, Report No. 408, p. 7.

[25] Quoted in *Congressional Quarterly Almanac*, 17 (1961), 146.

istration: "If we build anything like this, we are just going to be handed more and more of this business of fighting everybody's wars everywhere."[26] The belief that an increased ability to intervene would result in greater U.S. intervention abroad contributed to Congress's regular refusal to fund the FDL's.

It should be noted, without judging the sincerity of conviction underlying the rhetoric, that Congress has publicly explained individual budget actions affecting particular weapons systems programs in terms of their relation to national security.

The distribution of Congressional activity among budget categories indicates a programmatic orientation toward the defense budget. That orientation also should be reflected in Congressional responses to perceived changes in the international environment. Variations in the level of Congressional budget activity during the past eleven years should be associated with prominent events relevant to U.S. defense policy.

The mean absolute difference of the 28 line-items for each year is illustrated in Figure 1. As can be seen, Congressional activity was relatively high in both the beginning and the end of the period while the middle years were characterized by somewhat fewer or smaller changes by Congress in the President's budget request.

Explanations for the two dramatic changes which are observed may be divided into two general classes: (1) efforts to modify policies in response to perceived changes in the international environment, and (2) considerations of domestic politics reflected in Presidential elections. Unfortunately, not only is there a substantial interrelationship between the two—perceived changes in the international situation usually are reflected in their consequences for the domestic political scene—but changes of both sorts occurred nearly simultaneously during the time period under study. Nevertheless, the observed trend is consistent with the "programmatic" explanation suggested above.

The relatively high level of Congressional activity in the beginning of the period occurred during the last two Eisenhower defense budgets of fiscal 1960 and fiscal 1961. It was a period of the "missile gap" and the leaking of the Gaither Committee Report, when the doctrine of massive retaliation was losing its domestic credibility, and the nation appeared to be militarily unprepared. Congressional activity, which mostly took the form of increases in the President's budget requests, may have been a response to these changing perceptions of the security of the United States. As Goss concluded:

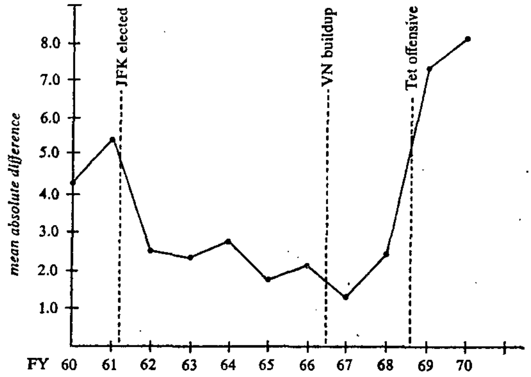

Figure 1. Level of Congressional Activity

> [Congressional] increases to weapons procurement requests have been associated with the perception of an increased threat and therefore have been justified on the grounds of needs of *national security*.[27]

The downturn in Congressional activism on the defense budget coincided with the first Kennedy budget (actually an amended Eisenhower budget). Perhaps the newly elected Democratic Congress was simply granting the requests of the narrowly elected Democratic President. But it also is true that the first JFK budget was highly responsive to the perceived shortcomings of the existing defense posture; substantial increases in *both* nuclear strategic and conventional forces were requested of the Congress. There was little left for Congress to change, and Congressional activism accordingly declined. The several international crises which punctuated these years also may have stimulated a "rally 'round the flag" attitude in Congress, making it reluctant to challenge the President on defense matters.

The level of Congressional activism declined further with Johnson's and the Congressional Democrats' landslide victory in 1964. There was, in any case, little indication of Congressional dissatisfaction with the President's defense budget requests: The budget was high but relatively stable, our superiority over the Soviets seemed assured, and, with the signing of the Limited Test Ban Treaty and reciprocal reductions in defense spending, the era of *détente* ap-

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[26] U.S. Senate, Committee on Appropriations, *Hearings on Department of Defense Appropriations for Fiscal Year 1968*, Part 1. 90th Congress, 1st Session, 5(1968) 7, 359–60.

[27] Goss, "Congress and Weapons Procurement," p. 13.

Table 7. Distribution of "No Change" Outcomes, by Fiscal Year

Fiscal Year	1960	1961	1962	1963	1964	1965	1966	1967	1968	1969	1970	
%	10	06	12	06	10	12	19	13	09	01	01	=100%
N	(7)	(4)	(8)	(4)	(7)	(8)	(13)	(9)	(6)	(1)	(1)	=(68)

peared to be dawning. In its review of the FY 1965 defense budget, the House Appropriations Committee observed: "This year, more so than in many previous years, the Committee has adopted to a large degree the program presented in the budget. . ."[28]

Congressional activism declined to its lowest level in the budgets immediately following the large-scale buildup of U.S. forces in Vietnam. The small and infrequent Congressional changes in the defense budget during this period may have reflected its confidence in the conduct of the war. The dramatic increase in Congressional activity (which reflected sharp *reductions* in the President's budget) occurred in the first defense budget to follow the Tet offensive of January, 1968. Explanations for this change in Congress's budget activity seem to be uncomplicated by partisan considerations: The 1968 elections were still several months away, Johnson had already removed himself, and the level of Congressional activism persisted after the election into the first Nixon defense budget.

These variations in budgetary activism may reflect Congress's changing assessment of the President's handling of national security. It is as if when Congress's confidence in the President is high, it makes few and minor changes in his budget. Larger and more frequent changes in the President's budget requests may signal Congress's dissatisfaction with his performance and its determination to take a more active role in defense matters.

The distribution of "No Change" outcomes, seen in Table 7, supports this general picture. The incidences of "No Change" are concentrated in the middle of the period when, it appears, Congress was prepared to support the President in his conduct of defense in general, and the war in Vietnam in particular. Of the "No Change" outcomes, 44 per cent are concentrated within the 27 per cent of the period represented by the Vietnam buildup (fiscal 1965–1967).

Congressional confidence in the President was waning in Eisenhower's last year in office, and under Johnson, was severely jolted by the experience of Tet. As can be seen in Table 7, Congress made frequent changes in the President's request during those years. Only 8 per cent of the "No Change" outcomes occurred during that 27 per cent of the period (FY 1961, 1969, 1970). In fact, only one minor line-item in twenty-eight was left unchanged in fiscal 1969 (Navy Reserve Personnel) or fiscal 1970 (Marine Corps Reserve Personnel).

The trend of Congressional activity over time supports the view that budget actions are inspired by program preferences. While Congress occasionally has indulged in across-the-board reductions to "encourage more efficient management," most of its decisions are based on judgments relating weapons systems to its perception of the nation's defense requirements. Thus, many of the changes made in the last Eisenhower budgets were designed to achieve greater "balance" in our force structure to enable the U.S. to fight non-nuclear wars. In fact, Congressional budget increases earmarked for Army modernization and airlift may be viewed as anticipating, by two years, JFK's efforts to move to a strategy of "graduated response."

Likewise, Congress's refusal to fund the fast deployment logistic ship program (FDL's), was publicly justified in terms of relating weapons programs to national defense policy. Critics may not applaud Congress's policy choices, but it does seem that Congress has, in the recent past, made such deliberate program decisions.

The House and Senate in Defense Appropriations

If it is true that Congress can and has made budget decisions based on program preferences, it becomes important to identify the relative importance of the two Congressional actors in the appropriations process, and to locate particular points of leverage available to an extra-Congressional participant seeking to influence the budgetary outcome.

Since the appropriations agreed to in conference and enacted into law are the culmination of Congress's role in the budget process, it might seem reasonable to judge which house is more dominant by comparing how frequently the conference report is equal to the Senate appropriations with how often it is equal to the House figure.[29] As can be seen in Table 1, the conference report is equal to the appropriations voted by the Senate about three and a half times as often as it is equal to the funds voted by the House.[30] On this basis, the locus of power and focus of effort seem obvious.[31]

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[28] Quoted in *Congressional Quarterly Almanac*, 20 (1964), 156.

However, as Fenno has cautioned: ". . . dominance in the conference committee must never be confused with dominance in the appropriations process as a whole."[32] It must be remembered that matters are raised in conference if and only if the Senate has made changes in the House version of the bill; issues to be resolved in the conference may be no indication of the extent to which the President's request has been changed.[33]

An alternative approach to this problem is to begin by comparing the number of individual changes[34] the House makes in the President's request with the number of Senate alterations of the House version. Changes made by both houses were examined for a subset of the data comprising the period FY 1966 through FY 1970.

Most of the changes which appear in the final Congressional version of the Defense appropriations bill were made by the House. The Senate made 161 individual changes in the House appropriation bills during the fiscal years 1966 through 1970. The House, however, made 344 separate changes in the President's budget *which were accepted by the Senate* during its consideration of the defense appropriations. That is, for every change made by the Senate in the House bill which required consideration by the conference, *two* House changes, accepted by the Senate, became part of the final appropriation without ever entering into the conference bargaining process. Only about 30 per cent of all Congressional changes were reviewed in conference. Thus, based on the number of changes in the president's request, the House plays the larger role in Congress.

Part of the explanation for this finding lies in the differences between the self-defined roles of the House and Senate in the budget process. As Fenno has noted in his study of domestic spending, the Senate Appropriations Committee does not scrutinize the entire budget request, as does the House. With its more limited resources, the Senate tends to concentrate on changes made by the House and primarily on those House actions that are appealed by the agency concerned.[35] "When an agency does not appeal a House cut, Senate appropriations committee members will take no further notice of it."[36]

In nearly every case in which the Senate did not respond to a House change, the DOD previously indicated acceptance of that change and requested of the Senate the amount appropriated by the House: Of the 344 House actions accepted by the Senate, 306 were instances in which there was no DOD reclama (appeal). Although most of the changes in the appropriations bill are made by the House, the House's role is, in one sense, influence by default. It may be accurate to say that the House dominates the Congressional appropriations process (since most changes in the President's request occur in the House), but this does not mean that the House prevails in its struggle with the Senate. The fact is that there are minimal opportunities for differences. The incidence of DOD reclamas, and the factors leading to the executive branch's decision to appeal a particular House change, emerge as central elements in any explanation of Congress's treatment of the defense budget.

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[29] For a similar procedure, applied to nondefense spending, see Fenno, *Power of the Purse*, pp. 661–670.

[30] This is somewhat greater Senate dominance than Fenno observed for nondefense spending (p. 663).

[31] Cf. David J. Vogler, "Patterns of One House Dominance on Congressional Conference Committees," *Midwest Journal of Political Science*, 14 (May, 1970), 303–320.

[32] Fenno, p. 663.

[33] There is another problem involved with the casual comparison of conference outcomes with House and Senate dollar amounts. Consider the following hypothetical example: (1) the House makes four individual cuts in a particular line-item; (2) the Senate makes no changes in any of the four decisions taken by the House, i.e., accepts the House's four cuts, but it reduces the President's request for another individual component of the same line-item; and (3) only the Senate's change is considered in conference (since that is the only difference between the bills of the two houses), and the House yields to the Senate. The result would be that the Senate appropriated less money than the House, and the appropriation to emerge from the conference would be exactly equal to the Senate's, i.e., it would be scored as a "straight Senate victory." It seems inappropriate, however, to talk about a Senate "victory" when there seems to have been little if any conflict between the two houses. The Senate accepted all of the changes proposed by the House, and the House, in turn, accepted the change proposed by the Senate. While, strictly speaking, the Senate did dominate the conference—the House did yield to the Senate—it cannot be said that the Senate dominated the Congressional appropriations process.

[34] Each of the 28 line-items considered is in reality a composite of several individual decisions. Thus, Congressional changes in budget requests for new destroyers, aircraft carriers, and attack submarines, appear, in aggregate, as a Congressional change in the line-item "Shipbuilding and Conversion, Navy," which in turn appears under the Procurement Appropriations title.

[35] Fenno, p. 534.

[36] Fenno, p. 569.

The most obvious fact is that the Defense Department does not appeal the majority of House changes. The general reluctance to appeal may be due in part to a desire to minimize friction with the Congress and a belief that challenging all or most House changes, as a matter of course and regardless of importance, will probably antagonize the Senate and will certainly antagonize the House. There is probably a general rule to restrict reclamas to cases of serious damage to very important budget items.[37]

All budget reductions are not equally detrimental to the interests of the military services (where the initiative to appeal usually resides). Although the House was very likely to cut most of the line-items in the President's request during the 1960s, these actions were not necessarily antagonistic to the priorities of the services. The services did not always strongly resist cuts in their budgets, e.g., conventionally powered guided missile destroyers and fast deployment logistic ships. Moreover, such reduction in low-priority items (low from the perspective of the service) frequently obscured increases over the President's budget in particularly esteemed service programs, e.g., nuclear powered guided missile frigates and "quiet" submarines. Establishing which of the houses is the more generous patron of the services cannot be done simply by comparing the size, in dollars, of Congressional budget changes.

There is undoubtedly also, the simple rule to appeal only when you think it will do some good. Since most House changes were *decreases* in the President's request, a reclama to the House decision represented an appeal to the Senate to restore the cut. The Senate usually could be depended upon to increase the House appropriation through FY 1966. Beginning in FY 1967, however, the Senate took a more critical attitude toward defense expenditures and the House figure exceeded the Senate appropriation for several of the line-items. By FY 1969, the House was voting larger appropriations than the Senate for most line-items. In view of this change in the temper of the Senate, reclamas may have decreased in number simply because they were perceived to be futile exercises.[38] (The recent inclination of the Senate to vote *less* money than the House may require a serious modification of any models of the budgetary process which assume that the Senate restores House cuts.)

It is clear that the House is responsible for a larger *number* of changes in the President's request than is the Senate. But this is not necessarily to say that the House has made the most *important* changes. It can be argued that the issues that must be resolved in the conference committee are primarily those in which the actors are most interested and to which they are most intensely committed. Identifying the house which was more successful in conference may indicate which house was more influential when it mattered. We therefore return to our comparison of the conference appropriations with the House and Senate versions.

As was noted above, the dollar amount recommended in the conference report was equal to the Senate appropriations about three and a half times as often as to the House figure. It may be argued, however, that occurrences of such "straight" House and Senate "victories" are more indicative of weakness of commitment by one house or the other, rather than of relative political power and domination. "Compromises" may signal an intensity of commitment in each house sufficient to require bargaining and reconciliation in the conference.

Cases of compromise shown in Table 3 were allocated to either the House or Senate according to which house's appropriation bill was closer (in dollar amount) to the appropriations recommended in the conference report.[39] These allocated compromises combined with the straight House and Senate victories are shown in Table 8. When compromises are included, the gap between the houses narrows to the point where the conference report is closer to

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[37] One reader of an earlier version suggested that the House Committee decides on the dollar amount of the contemplated reduction and then informally negotiates with the military services about which parts of the budget will absorb the cuts. Once an agreement has been reached, appeals to the Senate to restore the cuts would represent a default on the bargain; hence the low incidence of reclamas. Implicit in this line of argument is the hypothesis that Congress has no independent policy preferences but merely acts as a conduit for another's priorities, in this case the military services. The strong version of the hypothesis, viz., services distribute budget reductions among themselves in proportions equal to their shares of the defense budget, was not supported by the record of the past eighteen years. The general notion, however, is consistent with the data and certainly warrants further investigation.

[38] If this is true, then the data subset for the period FY 1966-FY 1970 may be unrepresentative of the total time period. A less rigorous analysis of individual budget decisions for the earlier period, however, yields the same general picture.

[39] Comparing dollar amounts is a crude and may be a misleading indicator of a victory in the conference committee: "If, for example, one group is willing to give up an item costing $50 million to get some especially valued item costing $10 million, then dollars and cents measures will be inadequate to define winning" (Fenno, p. 661).

Table 8. Distribution of Appropriation Outcomes, with "Compromises" Divided between House and Senate

Conference Closer to:	Personnel	O&M	Procurement	RDT&E	Total
House	06%	16%	28%	12%	15%
Senate	27%	46%	38%	48%	37%
	(109*)	(63*)	(87*)	(42*)	(301*)

* In seven cases, the conference report was equidistant from the House and Senate appropriations.

the Senate figure a little more than twice as often as the House figure.

We have already seen, moreover, that certain budget categories appear to attract more Congressional interest than others. Based on the high concentration of "No Change" outcomes and the infrequent occurrence of compromises, the Personnel category was judged to attract rather low attention in Congress. Yet nearly half of the Senate's straight victories concerned Personnel matters (See Table 3). Thus, the appearance of overwhelming Senate domination is largely the result of almost complete control of that budget category which experiences the fewest and smallest Congressional changes.

Although Senate victories outnumber House victories in all four budget categories, the gap between the two houses narrows considerably in the case of the Procurement budget, the category in which all of the participants appear to have the greatest stakes. Moreover, House victories were heavily concentrated in Procurement (index of concentration = 1.83). Thus the House appears to concentrate its efforts and successes where it matters most to most of the actors.[40] Conversely, the preponderance of Senate victories in conference is, to some extent, inversely related to the importance of the budget category.

In view of this analysis, the finding that Senate victories substantially outnumber House victories must be qualified. The Senate largely controls those parts of the budget which are least controversial. House victories are concentrated in the two areas which the participants appear to consider more important, and are nearly equal to the Senate in the most important appropriations title, viz., Procurement.

The 161 individual differences reconciled in conference between FY 1966 and FY 1970 were analyzed in order to probe the relative influence of each house in the conference committee. The most striking feature of these 161 differences is the surprisingly small number of "split-the-difference" compromises which occurred. Only twenty of the individual decisions represented such compromises. In 80 per cent of the cases, either the exact House figure or the exact Senate figure was the appropriation which emerged from the conference.[41] It appears from these figures that *logrolling* is the dominant form of conflict resolution in the conference.[42] The House either accepts the Senate position, or the Senate accepts the House position; very rarely do they fix on a figure which is neither. Conflict is resolved by trading concessions on separate individual decisions. At the level of the individual budget decisions, there are very few compromises and a great preponderance of straight victories. What appear to be "split-the-difference" compromises at the level of the line-items are usually the sum of the House victories and Senate victories on the component decisions of that line-item.

Analysis in terms of House or Senate "victories" connotes an atmosphere of struggle between the houses over particular items. Fenno's description of the conference highlights the participants' expectations of conflict.[43] But the preponderance of logrolling solutions suggests that conferences on defense appropriations may be less stressful, more friendly encounters than one might expect.[44]

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[40] It seems reasonable to credit to the House those of the outcomes labeled "same" which were accepted by the Senate, even though the Defense Department appealed them. If this is done, House "victories" are concentrated in the two most "important" budget categories. Moreover, the gap between Senate victories and House victories is substantially narrowed both in Procurement and RDT&E.

[41] In thirteen cases, the Senate accepted a House change and then added to it, the sum of the two actions being the appropriation reported by the conference. Thus, the House may have cut $100 million from the President's request and the Senate an additional $50 million from the House bill. The final appropriation would represent a decrease of $150 million from the President's budget request. Although the case could be made that such outcomes ought to count as Senate victories, they were classified separately in this analysis in the "both" category.

[42] See Wildavsky, *Politics of the Budgetary Process,* p. 61: "One way to secure agreement is to swap items in dispute."

[43] Fenno, p. 625.

[44] Cf. Jeffrey L. Pressman, *House vs. Senate: Conflict in the Appropriations Process* (New Haven: Yale University Press, 1966), p. 61.

The Senate's record of victory in the conference committee is impressive. Appropriations reported out of the conference were closer to the Senate figure because the House usually yielded to the Senate's position at the level of the individual budget actions. The House accepted the Senate figure 57 per cent of the time (or 65 per cent if cases in which the Senate added to a House change are included) out of the total 161 differences. The Senate accepted the House figure in only 23 per cent of the cases.

Although House victories and instances of compromise were concentrated in those cases in which the Defense Department made reclamas,[45] the Senate prevailed more often than the House in *every* category considered. Moreover, the Senate was inclined to act on its own initiative, without the stimulus of a Pentagon appeal: Fewer than half the Senate changes were preceded by a reclama. When the Senate changed the House version of the defense appropriations bill, those changes were very likely to appear in the public law submitted for the President's signature.

Although the House made many more changes in the administration bill, the Senate has not been a second-class participant; the latter usually has had its way in conference. Although a significant proportion of the Senate's victories were on items to which neither the House nor the executive branch were intensely committed, the Senate did prevail more often than the House in every category, from least to most important. Any confusion on the part of the would-be activist seeking points of leverage is justifiable. Nevertheless, the Senate's record of success in the past, combined with its recent history of economy and skepticism, support the observation that "Senate deviations from the norm seem to be the bellwether of developing trends."[46]

Summary and Conclusion

The widely held notion that an uncritical Congress passively grants whatever appropriations are requested by the President and the Pentagon, does not do justice to the reality of the pattern of the Congressional changes in the defense budget, nor does it illuminate the past and potential Congressional role in influencing the content of defense policy.

Defense budget requests in the 1960s appeared to emerge unscathed from the Congressional appropriations process primarily because Congress made small and infrequent changes in the Personnel and O&M titles which together constitute more than half the budget. Congress has focused its energies and concentrated its attention on Procurement and RDT&E—those parts of the budget primarily concerned with present and future weapons systems. This Congressional activity has been obscured in discussions based on total DOD annual budgets.

Two classes of speculations for the observed distribution of Congressional activity have been offered: budgetary and programmatic. It is the programmatic explanations that have the greatest potential significance for defense policy. Congress's changes in the defense budget can readily be explained in terms of its dissatisfaction with defense posture and policies, and its substantive efforts to improve them. During the 1960s, Congressional policy preferences (which prevailed more than once) were different from those of the President and often resembled those of the military services. Indeed, case studies would probably reveal that the military has been the source of Congress's defense policy objectives.[47] But the lesson for the future is that in voting defense appropriations, the Congress is concerned about more than *how much* money is spent. It also has evidenced a willingness and desire to influence the content of national security policy. Congress in the 1960s did not docilely accept and ratify the defense policy decisions of the executive. On the contrary, it played a relatively lively role in particular policies, an activism whose level seemed to be associated with a perceived inadequacy of presidential decisions and actions. Congress can influence in the future, as it has influenced in the past, the *content* of defense policy by means of its control of appropriations.

Most of the changes made in the President's budget request by the House were accepted by the Senate, were never discussed in the conference, and were retained in the final bill. This would make it appear that the defense priorities of the House are likely to be those which prevail in the final Congressional act. We have seen, however, that most such House actions (at least during the latter part of the decade) went unappealed by the Pentagon. The relative dominance of the House in the Congressional appropriations process becomes more difficult to judge in such circumstances. Moreover, when the Senate did make changes in the House bill, the House tended to yield to the Senate's position in the conference. In addition, the very recent change from the Senate's traditional role of dependably restoring House cuts to its new posture of being even more critical than the House of defense spending injects a further element of unpredictability into House-Senate relationships.

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[45] It would be inaccurate to posit any direct causal links between the presence or absence of a Defense Department reclama and the probability of a Senate victory in conference. The existence of a reclama is more likely to indicate that the matter under consideration is relatively important to the Pentagon; it probably is relatively important to the House as well. The presence of House victories and compromises probably indicates a greater intensity of commitment to its position and less willingness to yield to the Senate.

[46] Goss, p. 12.

[47] As the *Congressional Quarterly Almanac* observed in 1961: "The Senate Appropriations Committee's actions on long-range bombers matched almost exactly recommendations made to the Committee by Air Force Chief of Staff Curtis E. LeMay . . ." p. 146.

Congressional policy preferences, and relations among the houses in the course of the appropriations process, appear to be in a state of flux. Consequently, the record of the past decade may be a poor indicator of which house is likely to be more sympathetic to a reordering of defense priorities as well as which house is likely to be more influential in determining the content of final appropriations bill. The record of the 1960s does reliably indicate, however, that influencing the direction of defense policy is not a novel role for the Congress. The inclination and mechanism for Congressional intervention in matters of national security already are present. The direction and results of that inclination have yet to be determined.

Appendix

Line-items Included in Analysis	*Line-items Excluded from Analysis*
MILITARY PERSONNEL	
Military Personnel, Army	
Military Personnel, Navy	
Military Personnel, Marine Corps	
Military Personnel, Air Force	
Reserve Personnel, Army	
Reserve Personnel, Navy	
Reserve Personnel, Marine Corps	
Reserve Personnel, Air Force	
National Guard Personnel, Army	
National Guard Personnel, Air Force	
	Retired Pay, Defense
OPERATIONS AND MAINTENANCE	
Operation and Maintenance, Army	
Operation and Maintenance, Navy	
Operation and Maintenance, Marine Corps	
Operation and Maintenance, Air Force	
	Operation and Maintenance, Defense Agencies
Operation and Maintenance, Army National Guard	
Operation and Maintenance, Air National Guard	
	National Board for the Promotion of Rifle Practice, Army
	Claims, Defense
	Contingencies, Defense
	Court of Military Appeals, Defense
PROCUREMENT	
Procurement of Equipment and Missiles, Army	
Procurement of Aircraft and Missiles, Navy	
Shipbuilding and Conversion, Navy	
Other Procurement, Navy	
Procurement, Marine Corps	
Aircraft Procurement, Air Force	
Missile Procurement, Air Force	
Other Procurement, Air Force	
	Procurement, Defense Agencies
RDT&E	
RDT&E, Army	
RDT&E, Navy	
RDT&E, Air Force	
RDT&E, Defense Agencies	
	Emergency Fund, Defense

A Theory of Political Ambition: Career Choices and the Role of Structural Incentives*

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Introduction

Political ambition lies at the heart of much of the political "lore" in the United States. As the politician rises to new positions of power and prominence, his motives for seeking political advancement become subjected to intense public scrutiny. Most politicians try to promote the fiction that their motives are unsoiled by private ambition, that all they wish is to serve the public and to pursue the "public good"; but in the end, almost all American politicians appear somewhat suspect to the public at large, suspect in ways one seldom associates with corporation executives or those in other careers.

With all the public concern about excessive ambition, however, political ambition has not been a primary research subject for recent students of politics. There is, of course, a tradition in the discipline of political science of studying famous politicians, frequently through case studies that employ psychoanalytic methods, and we have, in addition, some rather extensive empirical findings about the "social backgrounds" of political leaders, but both approaches have failed to lead to an accumulation of knowledge about political ambition.[1]

The most significant exception to the general neglect of political ambition is the work of Joseph A. Schlesinger.[2] In his seminal study of political recruitment, Schlesinger suggests:

> The central assumption of ambition theory is that a politician's behavior is a response to his office goals. Or, to put it another way, the politician as office-seeker engages in political acts and makes decisions appropriate to gaining office. . . . It makes little difference to the theory of ambition whether men adopt the ambitions suitable to the office or attain the office because of their ambitions. It is sufficient to conclude that governors of New York will behave as though they were Presidential candidates while governors of Mississippi or South Dakota will not.[3]

Following an extremely insightful, yet introductory, analysis of the importance of political ambition, he proceeds through a careful and extensive examination of the recruitment patterns among the various offices in the United States. At no point, however, does he really focus on the actual interaction between the structure of offices and the ambitions of individual politicians. He thus leaves us with a strong case for the importance of political ambition but with little evidence of how it develops.

His work, however, does suggest several ideas that may prove of great value in the search for "theories" of political ambition. One such idea is his contention that a man's political motives and desires are molded by the availability of political "opportunities," and that such opportunities are structurally determined.[4]

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[1] One of the best examples of the "case study" approach of famous politicians is Alexander L. George and Juliette C. George, *Woodrow Wilson and Colonel House: A Personality Study* (New York: Doubleday, 1956). Studies of the social backgrounds of decision makers are quite common. See, for example, Donald R. Matthews, *The Social Background of Political Decision Makers* (New York: Doubleday, 1954; and W. Bell, R. J. Hill, and C. R. Wright, *Public Leadership* (San Francisco: Chandler, 1961).

[2] Joseph A. Schlesinger, *Ambition and Politics: Political Careers in the United States* (Chicago: Rand McNally, 1966). Several other exceptions to the general neglect of ambition include Heinz Eulau *et al.*, "Career Perspectives of American State Legislatures," *Political Decision-Makers: Recruitment and Performance*, Dwain Marvick, ed. (The Free Press of Glencoe, 1961); James David Barber, *The Lawmakers: Recruitment and Adaptation to Legislative Life* (New Haven: Yale University Press, 1965); and Chong Lim Kim, "Political Attitudes of Defeated Candidates in An American State Election," *American Political Science Review*, Vol. 64 (September, 1970), pp. 879–887.

[3] Schlesinger, pp. 6, 9.

[4] Schlesinger finds considerable consistency in the career patterns that politicians follow from state to state. He finds that some offices are substantially more likely to promote political advancement than other offices, and he characterizes these differences as the "opportunity structure" of the political system. Although one cannot prove it with these data, I suspect that the ambitions of individuals are strongly shaped by the availability of opportunities, and that this effect is manifested in the probability estimates that politicians assign to various alternatives.

tion in community elections. In general, the candidates from the "large" and "competitive" cities had to allocate more resources to obtain their councilmanic positions. The second consideration, which will become more important later in this analysis, is that the effect of city size is noticeably greater than the effect of the closeness of the vote. When city size is controlled, the closeness of the vote is still significantly related to both dependent variables, but size is obviously the more "powerful" of the two variables.

For many candidates there may be other costs attached to running for office, costs such as anxiety, tension, etc.[14] Time, money, and effort, however, are all resources that the prospective politician must allocate in order to obtain his office; and in the political choice process that confronts the prospective officeholder, the choice to spend these resources in office-seeking is also a choice not to spend the resources on other alternatives. For this reason they are an important component of the utility calculus of the potential office-holder when he must choose whether or not to seek political office.

The Elements of a Theory of Career Choices

Electoral Costs as a Political Risk. Students of politics have generally examined the electoral process in communities primarily in terms of the competitive characteristics of that process; i.e., the relative probabilities that candidates or parties will emerge victorious. And, with the exception of Schlesinger's efforts, most research on the competitive process has ignored the election as a risk taking venture in which candidates are forced to wager a portion of their resources in the pursuit of office.[15]

The importance of "risk" derives from the condition that the magnitude of such risk is partly determined by the structural characteristics of the electoral system in which the candidate runs for office. For the city councilmen, risk enters into their choice calculus at two points, when they originally decide to seek the councilmanic position, and when they must decide whether or not to pursue any further political offices. In both instances they are confronted with the possibility of losing the election; and even if they are fairly certain of winning, the necessity to campaign is likely to force them to make investments in politics that could be made elsewhere. And, as our earlier data indicate, the costs of campaigning appear to be strongly affected both by the size of the city in which the election occurs and by the competitiveness of the local elections.

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[14] Campaigning can be costly in a wide variety of ways. If a campaign is bitterly contested, the animosity, hostility, and rudeness of the campaign are all costs that the candidate and his family have to bear. It is difficult, however, to think of a way to aggregate costs such as these into a serious calculus of the "cost" of a campaign.

[15] Schlesinger, *Ambition and Politics*, p. 10.

Table 2. Relationship of Campaigning to City Size, to Electoral Competitiveness, and to Electoral Competitiveness Controlling for City Size

General Campaigning	Population			Average Closeness-of-Vote		
	Large %	Small %	N	High %	Low %	N
Considerable	72	37		56	28	
Moderate or Not Too Much	28	63		44	72	
Respondents	(110)	(259)	(369)	(233)	(124)	(357)

	Large Population Average Closeness-of-Vote			Small Population Average Closeness-of-Vote		
	High %	Low %	N	High %	Low %	N
Considerable	78	—		42	28	
Moderate or Not Too Much	22	—		58	72	
Respondents	(85)	(8)*	(93)	(139)	(106)	(245)

Question: "In general, about how much campaigning is there in city elections here in (CITY)? Would you say it is considerable, moderate, or not too much?"

* Too few cases.

Table 1. Relationship of Estimated Campaign Expenses to City Size, to Electoral Competitiveness and to Electoral Competitiveness Controlling City Size[13]

Estimation of Campaign Expenses	Population			Average Closeness-of-Vote		
	Large %	Small %	N	High %	Low %	N
$ 0–499	5	78		44	81	
$ 500–999	16	16		23	8	
$1000+	79	6		33	11	
Respondents	(105)	(273)	(378)	(223)	(123)	(346)

Estimate of Campaign Expenses	Large Population Average Closeness-of-Vote			Small Population Average Closeness-of-Vote		
	High %	Low %	N	High %	Low %	N
$ 0–499	4	—		57	81	
$ 500–999	12	—		22	13	
$1000+	84	—		21	7	
Respondents	(83)	(14)*	(97)	(141)	(119)**	(260)

* Too few cases.
** Rounding Error.

size of the community and the competitiveness of community elections should be related to the resources such as money, time, effort, etc., that candidates will have to spend on campaigning. This relationship can be partially substantiated with information gained from the interview with the city councilmen. In Table 1 both variables are correlated with the councilmen's estimates of the amount of money generally spent on campaigns in their cities. Both variables can be seen to be clearly related to the estimates of he amount of money that candidates for the councils spend on their campaigns.[12]

The same pattern of relationships can be found when the "cost" variable becomes the councilman's estimate of the amount of campaigning that generally occurs in his city. These relationships are shown in Table 2.

Two things stand out about these tables. In the first place, both of the "costs of campaigning," money and effort, are strongly associated with city size and with the degree of competician, when confronted with a decision, will examine the alternatives with which he is confronted, that he will evaluate those alternatives in terms of the likelihood of their occurrence and the value they hold for him, and that he will choose that alternative which yields for him the greatest expected value.[8] In this formulation, one assumes that the individual is attempting to select the best alternative for himself, i.e., that alternative which maximizes his expected value, but he may not, of course, always be correct in his choice.

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[12] The purpose of this table and the next is to stablish that a strong relationship exists between the osts of running for office and the two structural ariables. We have employed simple dichotomies and richotomies because a more detailed analysis of this elationship is unfeasible for our purposes. There are wo reasons for this. The more important of the two s that we cannot aggregate "costs" into a single index ecause they are not convertible into a common unit. n addition, it seems unwise to overestimate the ccuracy of this kind of recall data.

[13] In order to test this proposition we divided the ities into two categories, those above 30,000 in population and those below that number of people. The election of 30,000 as a cutting point was an arbitrary ecision: the only real concern was to choose a point hat seemed large enough to separate out the cities in hich the costs of running for office were significant, ut small enough so that we had sufficient councilmen n the large cities for the analysis. The measure of ompetitiveness for the city was the closeness of the ote among the various candidates for office. This measure was obtained by calculating the mean deviaon of the vote among all the candidates who reeived more than 15 per cent of the vote. The *mean deviation* (or absolute deviation as it is sometimes called) is computed by determining the sum of the absolute deviations from the mean number of votes per candidate and then dividing that sum by the number of deviations. The mean deviation is a measure of dispersion around the mean, but it differs from the standard deviation in that it weights every deviation the same, rather than weighting extreme scores more heavily. In almost all cases, the measure of the closeness of the vote, the mean deviation, was determined from five elections in each community, and the average was taken and is used here.

These mean deviation scores for five elections in each city were averaged to obtain a measure of the closeness of the vote for the city during a ten-year period. The Councilmen were then classified according to whether the city in which they were elected was "high" or "low" on the measure of the closeness of the vote. Virtually all of the cities are, in effect, multiple member districts. There were, on the average, more than two candidates for every office, and there were almost always more than two councilmanic positions at stake.

The dividing point between the "high" and the "low" competitive was the average mean deviation of 13 per cent. The range for the cities was between a low of 8 per cent on the variable and a high of 18 per cent. This cutting point was selected because it stood at the midpoint of the range.

Politicians, like everyone else, make errors; errors that can derive from an incorrect estimate of the probabilities attached to various alternatives or from an incorrect evaluation of the costs and benefits attached to a given alternative. These errors arise because information is almost always limited and expensive, and the decision-maker must generally arrive at his decisions through the process of an educated "guess." Thus, politicians can err in their choices even though they are trying to behave rationally.

In outlining the calculations of the rational office-seeker, we must begin by summarizing the basic variables in the decisional context:

B = the benefit, in *utiles,* that an individual will receive from achieving an office.[9]

P = the candidate's estimate of the probability that he can obtain an office should he attempt to seek it.

C = the cost required during a campaign to obtain an office.

$u(0)$ = the utility of an office for the individual prior to the election.

This calculus for the potential candidate is represented by the following statement:[10]

$$u(0) = (PB) - C$$

and the potential candidate will find it reasonable to seek office under the following conditions:

Condition I: The benefit of office-holding, (B), times the probability of obtaining the office, (P), is greater than the cost of obtaining that office, (C).

Condition II: The utility of office-holding, $u(0)$, is more valuable than the alternative ways the individual might invest his resources, i.e., $u(0) > u(A_i)$.

The first condition simply states that the expected utility of officeholding must be positive, while the second indicates that the utility of officeholding must be greater than the utility of the available alternatives. Both are quite obvious conditions, and they tell us little about the actual calculations of politicians. What is of interest here are the costs attached to seeking political offices, particularly those costs that are determined in part by the structural characteristics of the political system in which the individual seeks to run.

Campaign Costs and Political Structure

It is obvious that the rewards and costs of running for office in a city vary with a great many factors. Some factors, such as an individual's previous public prominence or an individual's need for public attention, are idiosyncratic, and their effect on the calculus of the individual is probably randomly distributed across cities. At issue here, however, are two factors that are structural in character—the size of a city and the degree to which a city's elections are competitive—and they probably have a systematic effect on the calculations of most of the individuals who seek public office in cities.[11] If their effect is systematic, i.e., they affect most individuals who seek public office, they may have a pervasive and critical impact on the social choice process that leads some men into the pursuit of public offices and leads other men to avoid public office.

In this regard the size of the electoral unit and the degree of electoral competition in that unity weigh heavily in determining the relative costs for candidates who seek office. Both the

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[8] For a detailed explanation of the approach, see R. D. Luce and Howard Raiffa, *Games and Decisions* (New York: John Wiley and Sons, 1957).

[9] The term "utile" refers to the abstract metric used in utility theory; and its value is specified in terms of the other abstract components of a theory, not in terms of some specific empirical measure such as dollars.

[10] The form of this function is a simplified version of what is a much more complex set of calculations. One simplification is that we are treating the calculus as if (P) and (C) are independent variables, which they most certainly are not. On the whole, the more a candidate spends on his elections, the more likely he is to win, that is, it might be rational for a candidate to increase his investment because it increases his probability of success at the polls. For our purposes, however, the interdependence between these two factors is less important because the analysis here is primarily concerned with the structural conditions that affect the value of (C) so as to establish some minimal value point below which (PB) cannot fall. The rather severe limitations in the type of data we have do not permit the more sophisticated and complicated analysis that would be required if we treated (P) and (C) as if they were interdependent.

[11] There are other structural characteristics which might similarly affect the recruitment of officeholders; nonpartisanship is one such characteristic, but it is not relevant here because all of the respondents were elected under nonpartisan forms of election. If some of the cities had had partisan elections, we would have had to take that factor into consideration.

The critical element here is the idea that a political structure acts on the choices of individuals so as to systematically affect all political office-seekers. Schlesinger's primary concern is with what he terms the "opportunity structure," and if the "opportunity structure" does have such a profound effect on the calculations of politicians, it is possible that other structural variables exert a similar systematic effect on these calculations.[5]

A corollary of the "structural effects" idea is the suggestion that the office-holder responds primarily to the immediate forces in his political environment rather than to factors that occur in the more distant past of the political actor. The average politician can hardly afford to develop elaborate plans concerning his political life. Rather, he probably tends to make decisions on the basis of the costs, benefits, and probabilities that operate at the time of his decision. If this is the case, then one should pay much more attention to the immediate circumstances surrounding a politician at the time of a decision rather than some set of factors in his childhood or elsewhere.[6]

These two ideas suggest the basis for a theory of political ambition. The theory rests on the idea that office-seekers attempt to behave in a rational manner in selecting among alternative offices; that rather than being driven by excessive ambition, they tend to develop ambition slowly as a result of their changing circumstances. The theory is partial, of course, because it excludes many variables that undoubtedly affect the development of political ambition in individuals; particularly those factors which move an individual to seek his first political office. The theory does, however, focus on a set of calculations and processes that appear to be central to the development of ambition in the politician.

The Setting for the Study

This is a study of 435 city councilmen in the San Francisco Bay Area, and the focus of the study is on the decision of councilmen either to retire from office, to seek reelection, or to seek other political or governmental offices. The study is based on data collected for the City Council Research Project, and it includes information gained through an extensive interview with the city councilmen and information on the political and demographic characteristics of the cities in which they hold office.[7]

All of the respondents in this study were officeholders at the time the research was conducted, and thus they had already exhibited a level of political involvement that is considerably higher than that of the average citizen. At the same time, I only have data on officeholders at one level of the political structure, the city council, and these city councilmen are probably not, on the whole, as ambitious as others who hold higher political offices. These considerations impose an obvious methodological problem: the data only reflect a restricted range of the variables under consideration. Restricting the range of one variable in an analysis normally has the effect of reducing the correlation between that variable and the other variables in the analysis. The implication here is that these tables will tend to underestimate the actual relationship one would find if the analysis was based on the full range variables. This point will be reiterated later because it poses some problems for interpreting these results.

The Rational Office-Seeker

The model employed in this analysis assumes that individuals attempt to behave in a rational manner, and that includes politicians who are in the process of seeking political advancement. The assumption of rationality, although generally accepted in economics, still provokes a strongly negative reaction among many political scientists, largely, it appears, because they do not understand the technical use of the term, as in game theory. The assumption of rationality in this context implies that the politi-

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[5] While Schlesinger can chart recruitment patterns, his data do not permit an examination of ambition directly. The study of political ambition is the study of the motivations of politicians and hence requires data on individuals.

[6] Such background factors are obviously important, but it remains to be seen whether their cumulative effect on a politician is very great. They are probably outweighed by more immediate concerns, particularly those related to the choices that are available at the time a potential candidate must make his decision.

[7] These data include open and closed items on the interview, census data, and election statistics gathered from each city. This research is, in several important respects, parallel with some findings reported earlier by this author; "A Theory of Professionalization in Politics," *American Political Science Review*, Vol. 64 (September, 1970), pp. 865–878. That article also employed a utility model, but the point of the article was to show that politicians tend to adopt attitudes that facilitate their ambitions, what we termed instrumental attitudes. The logic behind this phenomenon was similar to that reported here: some attitudes increase the probability of success in a career sequence, thus increasing the rate of return that an individual can expect from that sequence. In this article, however, our interest is the phenomenon of ambition itself, and our goal is to show how the political structure in a community can affect the level of ambition expressed by officeholders in the unit.

This information provides the basis for a general hypothesis relating risk to the two structural variables mentioned above.

Hypothesis I: The *risk* of running for office, (R), is an increasing function of the size of the political unit and of the degree of electoral competition in the political unit.[16]

Both parts of this hypothesis are essential for this analysis, but neither of them can be fully confirmed with the data available to us. The hypothesis implies that the costs of running for office are always greater than zero. This means that individuals would choose not to run for office unless the office was of some value to them, that is, if (PB) were greater than zero. These assumptions cannot be confirmed because I do not have data on the benefits that candidates receive from the act of running for an office; in other words, the benefits from campaigning that are independent of the outcome of the elections. The validity of the hypothesis will have to rest on whether or not the use of the hypothesis leads to predictions that can be confirmed with the data that are available.[17]

The magnitude of the risk for the politician depends on several factors. In the first place, the investment required for a campaign is likely to be substantial, and it constitutes only a portion of the investments required of the prospective candidate. In addition, however, the candidate may also have invested a substantial amount of time in another office or in other political activities, and prospective candidates may have to risk that investment in order to seek an office. And finally, any decision to seek an office involves an "opportunity cost"; the cost of foregoing other alternatives in order to seek a political office.

Although there are a variety of costs associated with holding office in these communities, the costs that result from elections constitute a significant portion of the total investment required by the councilman. Most councilmen, for example, have not accumulated a large investment in a political career that must be "risked" to seek the councilmanic position, at least not anything like the investment "risked" by House of Representatives members when they run for the Senate.[18] In addition, although the costs of being a councilman can be rather substantial, the councilmanic position is not a full-time job in any of the communities of this study. In most of the communities, the cost of holding office is minimal; but even if holding office involves some substantial investment, the magnitude of that investment is undoubtedly an increasing function of the size of the community, and the direction of that relationship only serves to reinforce the arguments in this analysis.

By assuming that the net investment of the councilmen is an increasing function of the size of his city and of the competitiveness of his city's elections, one can demonstrate that these investments dramatically affect the level of political commitment of those who seek public office. As the size of the investment required for an office increases (with city size and electoral competition), the less committed individuals should be more likely to drop out of the race for the office. Thus, we should find that the occupants of offices in the larger and more competitive cities are, in general, more committed to politics and to a political career than the occupants of offices in other communities. With these ideas in mind, we can outline the logic behind these effects.

$u(0) = PB - R$, where (R) represents the investment of the politician that will be lost if the politician loses his election. (2)

Proposition I: The greater the *risk* attached to a given alternative, (R), the greater that the *minimal value* of that alternative, (PB), must be for the individuals who pursue that alternative.

Proposition II: The lower the *probability* that an individual will succeed in achieving a given alternative, the higher must be the value of that alternative, (B), for some values of (P) and (B).

The first proposition restates a conclusion derived from the simple utility model outlined earlier; the value of an office, (B), must be greater than the risk of obtaining the office, (R); and as (R) increases in value, the minimal value of (PB) must increase correspondingly. The use of the term, *minimal value,* reflects the fact that (PB) can be and probably often is very much greater than (R); and that while (R) establishes a lower boundary on (PB), it does not in any way determine what the upper boundary might be.

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[16] The size of a city is also related to the degree of competition in city elections. Elections in the larger cities, on the whole, tend to be more closely contested than those in the smaller communities. For this reason city size must be controlled for when electoral competitiveness is employed an as independent variable. For empirical evidence of this relationship, see Gordon S. Black, *The Arena of Political Competition* (Bobbs-Merrill, forthcoming.)

[17] Some candidates undoubtedly do run for office, not because they expect to win but because it will benefit them in other ways. Local lawyers, for example, sometimes use office-seeking as a way of advertising, since advertising is normally forbidden under state law. The logic for such individuals is that they should attempt to minimize their total expenditures because they will benefit from all of the free publicity they derive from the election.

[18] Few of the councilmen had to "risk" another office to run for the council, and the ones that did resigned from relatively minor offices.

The second proposition also is a straightforward implication of our simple utility expressions that (P) and (B) are inversely related to each other in such a way that as either increases in value, the other will decrease in value (for some values of both variables). Because (PB) can be much greater that (R), however, (P) and (B) are not always fully interdependent; the degree of interdependence between (P) and (B) is a function of the ratio of (B) to (R).

If $(PB) > k$, where k represents some fixed value of (R), then

$$(B) = k/P \text{ as the } \lim_{(PB)\to k} (PB)/k = 1. \qquad (3)$$

and

$$(B) \neq (k)/P \text{ as the } \lim_{(PB)\to\infty} (PB)/k = \infty$$

Expressions (2) and (3) indicate that as the ratio of (PB)/R approaches one, (P) and (B) will increasingly become inversely related to each other; but as the ratio of (PB)/(R) increases, the degree of interdependence between (P) and (B) will decline. In empirical terms, this means that in the situation where the value of the office to the candidate is very much greater than the risk attached to achieving the office, a decline in (P) will not produce a corresponding increase in (B). On the other hand, where (B) is nearly equal to (R), any decline in the value of either variable will be associated with an increase in the value of the other variable such that the condition, $(PB) > (R)$ is satisfied.

Hypothesis I suggested that the risk of running for office is an increasing function of both the size of the political unit and the degree of the competition in that unit's elections. By implication, then, the following must also be true as shown previously.

Proposition III: The greater the size of the political unit, the higher will be the minimal value of (PB).

Proposition IV: The higher the degree of electoral competition in a political unit (less whatever relationship exists between size and electoral competition), the higher will be the minimal value of (PB).

Propositions III and IV suggest that the net value of an office, (PB), is an increasing function of the degree of electoral competition in a unit after controlling for the relationship between size and electoral competition. These relations do not predict all of the values of (PB), they only predict that (PB) cannot fall below some level determined by (R).

The Empirical Hypotheses. The remainder of the analysis will be concerned with the decisions confronted by these city councilmen at three levels: their original choice to seek office, their decision to attempt to retain their current office, and their decision to seek other political and governmental offices. These choices reflect two different levels of political commitment; the councilman's commitment to his current position which we will call *positional commitment*, and the councilman's commitment to seek other political and governmental positions, which we will call *progressive commitment.* The former is a reflection of the value the councilman has placed on achieving his present position and on staying in that office, while the latter reflects his generalized aspiration for other political or governmental offices.[19]

These two types of political commitment need not be highly correlated; that is to say, those who are progressively committed need not be committed to their current positions. A councilman may run for the council not because he was particularly concerned with local politics but because he wished to use the council as a springboard for other political offices. Likewise, a councilman who is highly committed to his position on the council may see that position as the limit of his political involvement.

One index of the councilman's commitment to his position is the strength of his original desire for the office. (Another, which is the subject of the next section, is the strength of his intention to seek reelection.) Because the respondents in this study are all incumbents, it follows axiomatically that the values they placed on their offices must have been greater than the risks they sustained to obtain those offices, i.e., $(PB) > (R)$, such that $u(O) > 0$. This is the case because $u(O)$ must be positive, that is to say, the councilmen must have expected to receive greater benefits from the office than the costs they sustained in seeking it. Knowing that (PB) must be positive, however, tells us little about the variation that exists in the values of (P) and (B). For such information we can refer to our earlier hypothesis that (R) is an increasing function of the size and electoral competitiveness of a community, a hypothesis that implies the following empirical hypotheses:

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[19] Schlesinger employs similar terms in his analysis, *Ambition and Politics*, pp. 10–11.

Hypothesis II: The councilman's desire for his office, (B), is positively related to the size of his city and to the degree of the competition in his city's election.

Hypothesis III: If the size of councilman's city is held constant, the councilman's desire for his office, (B), will still be related to the degree of the electoral competition in his city's elections, but the magnitude of that relationship will decline.

Hypothesis IV: If (R) is held constant, those councilmen for whom the outcome of the last election was uncertain will tend to express a greater desire for their office than those councilmen who felt they were certain to win. (See expression 3)

All four hypotheses follow directly from the cost/benefit model discussed earlier. Hypothesis II simply expresses the relationship between the value of an office and the risk attached to running, where risk is assumed to be an increasing function of both size and electoral competition. Hypothesis III indicates that electoral competition should have an effect on the value of the office for the councilmen that is independent of the effect of the size of the councilman's city. The last hypothesis expresses the *inverse relationship* that is expected between the preelection value of the office for the councilman and his estimated probability of winning in the election.

Before proceeding to a test of these hypotheses, we must make several points about the data employed here. In the first place, these empirical indicators are but crude reflections of the theoretical variables discussed in this research. For one thing, some of the indicators are based on recall data, and they are subject to all of the errors that are inherent in such data. In addition, the incumbency of all the respondents has the effect of skewing some indicators badly. (For example, the estimates of the likelihood that the councilmen might win their office originally are highly skewed toward certainty rather than uncertainty.) A final problem is that it is necessary to employ two surrogate variables as measures of electoral risk, size and the degree of electoral competition, because we lack complete (or additive) data on the costs of running for office.

These limitations in the data, limitations that are all too common in social science research, have caused me to follow two general rules in analyzing these data and testing the hypotheses. The first is that the method of analysis should be kept simple. (And simplicity is appropriate since we are not attempting to account for variance.) The second is that the theory in this research depends not on a single hypothesis but on a pattern of hypotheses, all of which must be confirmed before we can have any confidence that our theory has merit. With these considerations in mind, we can examine hypotheses II and III (see Table 3).

Table 3 reflects the result of asking the councilmen to evaluate their desire to be elected during their last campaign, and it is used here as a measure of (B). In both analyses, the table shows that the hypotheses are confirmed; moreover, when city size is controlled, both city size and closeness of the vote in elections are cumulatively and positively related to the councilman's *desire to be elected* to his office.[20]

Size, however, appears to be more strongly related than electoral competition to the councilman's desire to be elected. This finding was anticipated earlier when the variable of city size was observed to have a consistently stronger relationship to the two cost variables, money and effort.

The next hypothesis to be tested is the effect of the probability of winning on the councilman's original desire for the office he holds. Because the variable is highly skewed toward a high confidence of winning, the sample has been divided into two groups, one containing those councilmen who were nearly certain of winning and the other containing the remainder, who expressed varying degrees of uncertainty. In order to make this comparison, however, it is necessary to control as much as possible for the variable of electoral risk. This can be done by controlling for city size and for electoral competition.

As Table 4 indicates, in both the "small" and the "large" cities, the councilmen that were more certain of winning tend, on the whole, to place a lower value on the councilmanic position. The magnitude of this relationship appears to be greater in the large cities than in the "small" cities. We can now examine a similar table that uses electoral competition as the control variable.

The same pattern is apparent in Table 5 as in Table 4. Although the magnitude of the relationship is somewhat smaller than before, the councilmen who were more certain of winning tended to place a lower value on their office at the time of their last election than the councilmen who were less certain of winning. Although hypothesis IV is confirmed in both tables, this confirmation should be taken with caution because the number of cases in one column in each table is small. At the same time, the councilman's confidence of winning only measures a limited range of (P) because very few councilmen indicated that they thought they might lose. If the distribution of (P's) for the councilmen were less skewed, a greater effect from this variable could be expected.

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[20] I have employed the average closeness of vote because it gives a better measure of the general competitiveness of the community. Candidates probably form their expectations of what to expect in an election on the basis of what generally transpires in their city.

Although the predictions from the model appear to be confirmed in each instance, the factor of risk probably does not *cause* candidates to place a high value on the office they seek to obtain. Inasmuch as these risk factors are structural characteristics of the system, they must be faced by all potential candidates when deciding whether to run for office. In a system characterized by a large initial risk in running for office, only the more committed candidates are likely to run for office, that is, those who place a high value on the office. The factors that cause individuals to place a high value on an office lie elsewhere; in the importance of the office, in the probability that the office will lead to other more important offices, and in the background of the individual. All the model predicts is that the higher the risk of running for once, the more committed will be those who run for the office. This prediction is important, however, because it suggests that the structural characteristics of political units determine, in part, the level of the commitment of the men who hold office in those units. This appears to be one of the ways that the electoral system operates as a screening device; i.e., it can, under certain conditions, screen out the less committed potential officeholders.

The Decision to Seek Reelection

Once the individual has achieved office, he is faced with a second choice in relation to that office; should he seek reelection? Although one might intuitively expect that those who place a higher value on the office are more likely to seek reelection, this would be the case only if the risk of running again were the same for all councilmen, which it is not.

As shown earlier, those who place a high value on their office tend to face a higher risk should they decide to seek reelection, while those that place a lower value on achieving office also tend to face a lower risk in running for reelection. The implication for the model is that each factor must be considered in relation to the other, that the decision to run again will be directly related to the difference between the value placed on the office and the risk attached to running for reelection.

What we should expect to find is that if (R) is held constant, u(O) will be an increasing function of (PB). This means that when the two surrogate measures of risk are controlled, those councilmen who indicated they had "considerable" desire for their office will be more

Table 3. Relationship of Desire to be Elected to City Size, to Electoral Competitiveness, and to Electoral Competitiveness Controlling for City Size

Desire to be Elected	Population			Closeness-of-Vote		
	Large %	Small %	N	High %	Low %	N
Considerable	57	26		42	19	
Some	22	12		15	14	
Little	21	62		43	67	
Respondents	(90)	(242)	(332)	(208)	(101)	(309)

Desire to be Elected	Large Population Average Closeness-of-Vote			Small Population Average Closeness-of-Vote		
	High %	Low %	N	High %	Low %	N
Considerable	56	—		34	16	
Some	21	—		12	14	
Little	23	—		54	71*	
Respondents	(77)	(6)**	(83)	(131)	(95)	(226)

Question: "Which of the following statements most closely approximates your desire and efforts to be elected to the Council during your *last* campaign?

* Rounding Error.

** Too few cases for interpretation.

Table 4. Relationship between Desire to be Elected and Confidence of Winning in Last Election (Controlling for Size of City)

Desire to be Elected	Large Population Confidence in Winning			Small Population Confidence in Winning		
	Certain %	Uncertain %	N	Certain %	Uncertain %	N
Considerable	53	85		26	26	
Some	23	8		10	26	
Little	24	8*		64	49*	
Respondents	(68)	(13)	(81)	(190)	(39)	(229)

Question: "Which one of the following statements most accurately reflects your confidence in winning at the time of your *last* election? (I felt fairly certain of winning. I thought I could win, but I felt the election would be close. I thought the election could go either way. I expected to lose, but I thought if I worked, I could make it a close race. I felt fairly certain of losing.) The first two statements were grouped into the category of "certain" while all the rest were placed in the category of "uncertain."

* Rounding Error.

likely to seek reelection than those councilmen who expressed only "some" or "little" desire to be a councilman. Moreover, the relationship between the councilman's desire for his office and his intention to run again should be greater in the highest risk cities than in the lowest risk cities, because when (R) approaches zero, any positive value of (PB) will result in a decision to seek reelection.

Although the magnitudes of the differences in Table 6 are small, they tend to support the conclusion that when (R) is controlled (by controlling the size of the city and the degree of competition in elections), the original value placed on the councilmanic position by a councilman appears to be related to his intention to seek reelection. In the highest risk cities, the difference is 15 per cent; and in the intermediate risk cities, the difference is 11 per cent. The lowest risk cities do not follow this pattern, that is, the councilman's original desire for the office is unrelated to his intention to run again in the cities.

When these tables are viewed from another direction, however, the lowest risk cities do not constitute an exception to the prediction made above. An equivalent implication derivable from the model is that if (PB) is held constant, the risk of (R) running for office in a city, will be inversely related to the probability that a councilman will seek reelection. This proposition can be examined by comparing the councilmen who expressed "considerable" desire to be elected in both the "lowest" and "intermediate" risk cities and the councilmen who expressed only "some or little" desire to be elected in the same set of cities. The difference in the "some and little" categories is 18 per cent, whereas among the councilmen who indicated "considerable" desire to be elected the difference is only 16 per cent. This finding is consistent with the theoretical relationship between (PB) and (R). The higher the value placed on the office (PB), the more the risk of running for reelection (R) will have to increase before the risk of running for reelection is

Table 5. Relationship Between Desire to be Elected and Confidence of Winning in Last Election (Controlling for the Average Closeness of the Vote in City Elections)

Desire to be Elected	Closeness of the Vote: High — Confidence in Winning			Closeness of the Vote: Low — Confidence in Winning		
	Certain %	Uncertain %	N	Certain %	Uncertain %	N
Considerable	41	47		18	25	
Some	14	21		12	25	
Little	45	32		70	50	
Respondents	(167)	(38)	(205)	(89)	(12)	(101)

greater than the value of holding the office, that is to say, for (R) to be greater than (PB) so that u(O) is less than or equal to zero.

These data and the tables presented earlier suggest that the utility calculus outlined at the beginning of this analysis may prove a powerful tool in accounting for the career decisions that politicians must make. The real test of the model, however, is whether it permits us to predict the circumstances under which politicians are likely to develop that progressive commitment to politics, that we call political ambition.

A Theory of Political Ambition

The Search for Other Offices. Let us assume for a moment that a political system is composed of a set of positions that differ both in the benefits and the costs attached to those positions and in the investment required to achieve those positions: being a governor, for instance, tends to be more costly in terms of time, effort, and money than being a mayor of a city of moderate size. In order to induce men to seek more costly positions, those positions must be made relatively more rewarding than the less costly positions. This means that as the costs of an office increase, the benefits to be derived from that office must increase faster than costs, such that the net benefit from the more costly position is greater than the net benefit from the less costly position. If this were not the case, rational men would not seek to advance to more costly positions.

The price that a politician is willing to pay for a position is the sum of the investments that he is willing to make in order to obtain the position. These investments are made in two general ways; by actually holding an office in a given career sequence and by competing in the periodic elections that politicians face in the course of a political career.

These investments constitute a measure of the political commitment of individuals. As the size of the investment required for an office increases, the less committed individuals are likely to drop out of the race for the office. My intention here is to show that with an extension of the calculus outlined earlier, one can predict the circumstances under which individuals are likely to aspire to higher levels of office.

One way to approach this question is to formalize the concept of a career sequence. Assume that a political system is composed of a set of positions, $(X_1, X_2, \ldots, X_m)$, where X_i represents the ith position; and further assume that each of these positions has attached to it a set of costs, $((M_1, R_1), (M_2, R_2), \ldots, (M_m, R_m))$, where M_i represents the cost of *main-*

Table 6. Relationship Between Decision to Seek Reelection and Original Desire for the Office (Controlling for Size of City and Degree of Electoral Competition)

	Large Population					
	Average Closeness-of-the-Vote					
Intention to Run Again	High			Low		
	Considerable %	Some & Little %	N	Considerable %	Some & Little %	N
Yes and Probably	73	58		—	—	
No	27	42		—	—	
Respondents	(33)	(26)	(59)	(6)*	(6)*	(12)

	Small Population					
	Average Closeness-of-the-Vote					
Intention to Run Again	High			Low		
	Considerable %	Some & Little %	N	Considerable %	Some & Little %	N
Yes and Probably	67	56		73	74	
No	33	44		27	26	
Respondents	(39)	(72)	(111)	(15)	(72)	(87)

Question: "Do you expect to continue to run for the city council?"
* Too few cases to interpret

taining the ith position. Assume, in addition, that each potential candidate has a utility function such that $u(X_i) = (PB_i) - (R_i)$, where B_i is the value of the ith position and is composed of the sum of the benefits of holding office, (b_i), minus the costs of holding office, (M_i); i.e., $B_i = (b_i - M_i)$.

The sum of the costs of holding office (M_i) contains within it a set of costs which are uniquely political. For example, a politician needs to learn how to operate in a political position, he needs to know to whom he must go in a constituency to obtain political support both for himself and for policies he wishes to pursue; and he needs to know which coalitions are possible and which are not. Finally, he must become familiar with the laws of his system, the rules of procedure in his political arena, and the informal norms that regulate the interpersonal relations among politicians.

Similarly, the risk of obtaining a position also contains a set of costs which are common to politics but which are less frequently found in other occupations. The potential candidate will have to become well enough known to his constituents so that they will support him at the polls. As before, he needs to know the structure of coalitions in his constituency, the groups from which he might obtain support, and those individuals who can provide him with financial support for his campaign. Finally, it would be useful to know how to run a campaign organization, how to handle himself in public, and what themes would be most useful to emphasize in seeking electoral support.

Most of these types of experience and knowledge can only be acquired in a single way, by investing in the political process in the area in which the candidate seeks to obtain a political position. At the same time, each investment that the individual makes in the political process in his political unit is an investment not only in the office he seeks or holds *but also potentially in the other offices or positions eventually open to the individual.* For example, the political novice who seeks a particular office will have to make a greater investment to obtain that office than the individual who has had long experience with the politics of the political unit. This phenomenon we will call an *investment transfer,* and it occupies a central place in this theory of ambition.

Investment Transferability. As a first approximation of the investment process for politicians, let us assume that politicians evaluate the alternatives that face them in terms of the relative *rates of return* they can expect from these alternatives,[21] and further assume that their investment strategy will reflect their attempt to maximize their expected value from their net investments. Given the constraint that they can make a full investment of their resources, the attempt to maximize expected value is theoretically equivalent to the attempt to maximize one's average rate of return.

In the particular case at hand, all the individuals interviewed for this study were successful in obtaining a seat on the city councils in their respective cities. Since all of them could conceivably seek other offices, they each face three choices with regards to their future participation in politics: to decline to seek further office (at least in the short run), to seek re-election to the council, or to seek other political or governmental offices. They will choose that alternative with the highest expected rate of return, represented as follows:

Outside Alternative	Council Alternative	Other Political Alternative
$\dfrac{(P_o b_o)}{L_o + R_o}$	$\dfrac{(P_c b_c)}{L_c + R_c}$	$\dfrac{(P_i b_i)}{L_i + R_i}$

Only a portion of that which an individual invests in politics can be applied to a nonpolitical alternative; and as the individual continues to invest in a political office, the portion of his net investment that can be transferred to a nonpolitical alternative, relative to his total investment, probably declines. The principal effect of his investment in one office, to the extent that it is transferable to other positions, will be to reduce the size of the additional investment he must make in order to obtain those positions.

By reducing the magnitude of the investment necessary for other offices $(L_i + R_i)$, the individual is, in effect, increasing the rate-of-return he can expect from those other offices. This increases the attractiveness of other political offices relative to nonpolitical aternatives, and the magnitude of that increase should be a function of the cumulative investment the individual has made in politics.

This process by which the rates-of-return of various political and nonpolitical alternatives change as a result of an individual's political investments is part of the political choice process for politicians. In any career sequence, if this analysis is correct, as the individual increases his investment in the career sequence, the value of the next step upward or the next goal will also increase relative to alternatives outside the career sequence. In a sense, the individual's investments tend to pull him further and further into the sequence even though he may not have originally intended to follow the route on which he now finds himself.

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[21] The concept of rate of return as employed here includes the individual's estimate of the probabilities of succeeding at the various alternatives. We will not attempt to explore, however, the complex problem of the portion of their resources that councilmen will invest in politics because we lack data on the councilman's nonpolitical alternatives. This has no effect on the logic of the argument discussed next.

This does not mean that his rate of investment must increase except insofar as the next position he might seek requires a higher level of investment, but his *cumulative investment* will increase because he continues to regularly invest in politics. It is this cumulative investment that makes nonprofessional alternatives outside the career sequence relatively less attractive. This proposition can be formalized as follows:

Proposition V: The greater the cumulative investment of the individual in political office-seeking, the greater will be the value placed on the offices to which the individual might aspire.

Of the variables mentioned earlier, size and competition both would seem to reflect part of the cumulative investment of these councilmen. Since the office of city councilman is nearly at the bottom of the office structure in this system, it seems likely that these individuals' electoral investments constitute a significant portion of their total investment in politics.

The essence of the question, however, is whether these two variables represent investments that are in fact transferable to other offices. For obvious reasons, city size probably reflects a set of investments that are transferable, both those investments made during elections and the investments made while holding office.[22] The costs associated with electoral competition may, on the other hand, not be transferable to other offices. In fact, the closely won campaign may prove detrimental to an individual's chances of seeking other offices, partly because of the antagonism generated during such campaigns and partly because a close election indicates that a sizeable portion of the electorate favors other candidates.

The variable of size operates in the expected manner; about 22 per cent more of the respondents in the "large" cities indicated they desired other offices than those in the "small" cities. At the same time, the closeness of the vote in elections is only slightly related to the councilman's ambitions when the variable of city size is employed as a control variable.

The councilman's confidence in winning also may affect his desire to seek other political or governmental positions, but the logic behind this relationship is somewhat different from that outlined above. One of Schlesinger's central points is that the probabilities of obtaining various offices are interdependent, so that success at obtaining one office alters, positively, the probability of obtaining other offices in a career sequence, i.e., $P_i = f(P_c)$.[23] If such probability estimates are interdependent, then the councilmen who were confident of winning their council seat might also be more likely to seek other offices. The reason for this is that any factor which increases the value of P_i also increases the rate of return that can be expected from the ith office.

Table 8 shows a rather strong and expected relationship between the councilman's confidence in winning his council seat and his aspiration to seek other offices. Councilmen who were *certain* of winning their councilmanic seats are also more likely to aspire to higher office than those who were uncertain of victory. Although this relationship is not a strict implication of our original model, it does fit the model if we accept the additional assumption that subjective estimates of achieving office by politicians are interdependent in the manner discussed earlier.

The final two variables in this analysis, the councilman's original desire for the councilmanic position and his intention to seek reelection, should both be related to the councilman's desire to seek other offices. The desire to

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[22] By introducing the concept of "transferability," we are opening up a relatively uncharted set of questions for analysis. Investments in public office, unlike those in stocks, are not readily translated into a common denominator such as money. The investments in politics involve rather amorphous quantities such as experience, knowledge, prestige, etc., and we know next to nothing about the general process through which such factors are aggregated by the individual. At the same time, however, the existence of an investment process (with some element of transferability) seems quite plausible and perhaps intuitively obvious. The data here seem to substantiate the existence of these phenomena, but we have only made a small and preliminary step toward delineating the full dimensions of the process.

[23] Throughout this paper we have conducted our analysis entirely with *subjective* probabilities, and we continue to do so here. Utility theory rests not on the real likelihood of a given outcome, but on the individual's estimate of what those probabilities are. An intriguing question, however, is the extent to which subjective estimates correspond to the objective events faced by candidates. I suspect, although this is simply conjecture, that candidates tend systematically to overestimate their electoral chances.

Table 7. Relationship of Political Ambition to City Size, to Electoral Competitiveness, and to Electoral Competitiveness Controlling for City Size

Desires Other Political Office	Population			Average Closeness-of-Vote		
	Large %	Small %	N	High %	Low %	N
Yes and Perhaps	55	33		41	34	
No	45	67		59	66	
Respondents	(102)	(294)	(396)	(247)	(149)	(396)

Desire Other Political Office	Large Population			Small Population		
	Average Closeness-of-Vote			Average Closeness-of-Vote		
	High %	Low %	N	High %	Low %	N
Yes and Perhaps	52	—		35	31	
No	47	—		65	69	
Respondents	(87)	(15)*	(102)	(160)	(134)	(294)

Question: "Are there any other political or governmental positions—local, state, or federal—which you would ike to seek?"

* Too few cases.

seek reelection is also an expression of a willingness to continue to invest in politics, and this is consistent with the discussion earlier which suggested that a portion of such investment could be applied to other offices and would increase the attractiveness of such alternatives by increasing the rates-of-return from those alternatives.

We expect a relationship between the councilman's original desire for his office and his desire to seek other offices, because a great many councilmen value their office more than the minimal amount required by the propositions outlined earlier. I suggested that the campaign created a kind of investment barrier, but that many prospective councilmen could value their office considerably more than they actually have to pay for it. That excess evaluation probably represents a kind of generalized commitment to politics, and it might be expected to be related to a higher level of value placed on offices up the career chain. Table 9 shows these two relationships. Both variables can be seen to be positively related to the councilman's desire to seek other offices.

What remains is to show that the three principal variables of this part of the analysis have a cumulative effect on the level of ambition demonstrated by the councilmen. Our argument has been from the beginning that such a cumulative effect should occur, that we should be able to demonstrate that each of the variables has an independent effect that is not canceled out by controlling for the other two variables. In Table 10 all three variables—the original risk of running for the council, the original value placed on the councilmanic position, and the councilman's intention to seek reelection—are correlated with his desire to seek other offices.

Table 8. Relationship of Political Ambition to Confidence in Winning in the Last Election (Controlling for City Size)

Desires Other Political Office	Large Population			Small Population		
	Confidence in Winning			Confidence in Winning		
	Certain %	Uncertain %	N	Certain %	Uncertain %	N
Yes and Perhaps	59	46		35	26	
No	41	54		65	74	
Respondents	(76)	(13)	(89)	(202)	(39)	(241)

Table 10 shows dramatically the existence of a cumulative effect when the three variables are employed together. The net shift across the whole table is 53 per cent, and the step increases across the table are fairly regular. The effect of each variable can be determined by calculating the average of the percentage increases between the low and high values of each variable, e.g., by comparing the small cities with the large cities, etc. The average increase attributable to size was 28 per cent; the increase due to the councilman's original desire for the office was 12 per cent; and the increase owing to his intention to run again was 13 per cent. Thus we find that all three variables have an independent effect on the councilman's ambition for other political and governmental offices.

Conclusions

The primary purpose of this paper was to show that the structure of the political system in which men are recruited for office can have a profound influence on the type of men who emerge as candidates. In order to accomplish this purpose, I have elaborated a model of political ambition based on the principles of utility theory; this model was employed to develop a set of empirical hypotheses that could be tested at least partially with the data available to us. The analysis of those hypotheses suggest several important conclusions:

(1) The structure of political systems may play a significant role in shaping the ambitions of the men who occupy offices within the system. Although we only dealt with two such variables—the size of a community and the degree of competition in community elections—our findings suggest that the structural characteristics of systems can shape both the risks that politicians face and the investments that are required to reach political office. In a sense, our findings complement those of Schlesinger, who suggested that the structure of political opportunities has a similar effect.

Although these structural variables were found to be associated with the values assigned to offices and the ambitions of our politicians we have avoided suggesting that these variables *cause* changes in the evaluations of politicians A more accurate and methodologically usefu interpretation is that structural variations create barriers that the aspiring politician must overcome. In this way the structure of a politica system acts as a filter that allows some types of individuals to move up through the system while others are either stopped in their progress or diverted in less risky and costly directions Thus, the system does not *cause* either ambition or success in a direct sense; what it does is to determine indirectly the kind of men whom we will find in various types of offices.

(2) A second conclusion, more tentative than the first, concerns our view of the socia choice process for the individual, using utility theory as an analytic tool. One implication is that the study of political ambition generally can benefit from an approach that assumes tha politicians attempt to behave in a rational manner in seeking their political aspirations. All of our tables were consistent with an interpretation that the career choice process for politicians is based on some simple elements of utility theory outlined earlier. This finding suggests that a rationalistic explanation of the career choice process may prove to be the road to an adequate theory of political ambition.

A second implication is that it may be desirable to seek the logic behind political choice in the immediate environment of the political actor rather than search his distant past for the motives and experiences of childhood. Much of the work on political recruitment has been focused on the social background or family experiences of the politician, and while these may have some impact on the career choices that politicians make, these findings suggest that more immediate forces play a powerful role as well.

(3) Our findings also suggest that political ambition develops in part as a result of the investments that politicians make in their politi-

Table 9. Relationship of Political Ambition to Intention to Run Again and to Original Desire for the Council Position

Desires Other Political Office	Desire to Be Elected			Intention to Run Again		
	Considerable %	Some & Little %	N	Prob. %	No %	N
Probably	50	35		48	38	
No	45	65		52	67	
Respondents	(101)	(186)	(287)	(191)	(96)	(287)

Table 10. Relation of Political Ambition to Size of City, to Original Desire for the Office, and to Intention to Run Again for the Council

	Large Population					
Desires Other Political Office	Considerable desire for the Council Intention to Run Again			Some or little desire for the Council Intention to Run Again		
	Prob. %	No %	N	Prob. %	No %	N
Probably	73	62		63	53	
No	27	38		37	47	
Respondents	(30)	(13)	(43)	(19)	(15)	(34)

	Small Population					
Desires Other Political Office	Considerable desire for the Council Intention to Run Again			Some or little desire for the Council Intention to Run Again		
	Prob. %	No %	N	Prob. %	No %	N
Probably	49	35		36	20	
No	53	65		64	80	
Respondents	(41)	(17)	(58)	(101)	(51)	(152)

cal activity. As a politician invests in one office, even if he has little desire at the time to seek higher offices, he is altering his evaluation of other offices in a potential career sequence. This alteration occurs because some part of his investment in politics is transferable to the pursuit of other offices, and the ability to transfer investments increases his expected return from higher offices. Thus, each step in a political career sequence alters one's evaluation of the other step: in that sequence, especially in regard to nonpolitical alternatives. As the politician's investment increases, his evaluation of political alternatives is likely to become more positive while his evaluation of nonpolitical alternatives is likely to remain about the same. The net effect is the development of higher levels of ambition in the politician. This description of the development of ambition differs markedly from the view that sees the politician as a driven man who decides his course early and plans his whole life accordingly. Perhaps there are such men, but we suspect they are a distinct minority. The tides of politics are too great to permit men to chart an undeviating route through the uncertain and troubled waters of political life.

The Committee Function: An Influence Equation[*]

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In legislative systems a bill is commonly considered and reported by an appropriate committee before it is considered (with a simple majority required for passage) on the floor of the house. Since motions on the floor (amendments et al.) frequently relate to bills, when speaking of a motion it is often apposite to refer to *the relevant committee.* With respect to a given motion and its relevant committee, the extent of committee agreement *on the floor* is obviously consequential. There exists a substantial volume of folklore on this point, particularly related to the special case of committee unanimity,[1] but for precision and generality, a mathematical relationship between the relevant committee's divisions on the floor and a motion's probability of passing (or not passing) is preferable. The purpose of this note is to present such a mathematical model.

A first approximation, a linear function in one variable is easily extracted from the data in Table 1: If P = probability of passing = $f(x)$ where x = proportion of relevant committee voting yea, then $f(x) = 0$ when $0 \leq x \leq 0.2$ and $f(x) = 1$ when $0.8 \leq x \leq 1$ are obvious albeit incomplete conjectures; a straight line extrapolation for the remainder of the curve yields $f(x) = 5x/3 - 1/3$ when $0.2 \leq x \leq 0.8$. This function yields the theoretical values given in the fourth column of Table 1.

The function $f(x)$ is a respectable approximation, as a comparison of the third and fourth columns of Table 1 nicely shows. However, although continuous, that function is not a smooth function—specifically, the function $f(x)$ lacks tangents at the points $f(0.2) = 0$ and $f(0.8) = 1$. Mathematically speaking, a function with derivatives at all points and numerical values similar to those of the function $f(x)$ is to be preferred. Fortunately, $P = g(x) = x^2/[x^2 + (1 - x)^2]$ is continuously differentiable and yields the values shown in the fifth column of Table 1.

The function $g(x)$, unlike the function $f(x)$, has the intuitively desirable property of being parsimonious with the limiting probabilities of one (1) and zero (0); specifically, it yields 0 if and only if $x = 0$ and 1 if and only if $x = 1$. Both functions are symmetrical with respect to the yeas and non-yeas; neither $g(x)$ nor $f(x)$ discriminates between the nay-saying and nonvoting members of the relevant committee. However, presumably, the probability of a motion's passing is adversely affected if (say) one-half of the relevant committee members vote nay rather than abstain. Thus a smooth function in two variables is desired.

If $P = h(x,y)$ where y = proportion of relevant committee not voting, then the proportion of the relevant committee voting nay is a derived quantity $z = 1 - x - y$. Since committee members (as an empirical matter of fact) tend to abstain less frequently than other members of a legislative body, the numerical value of y is often quite small. This suggests that the smooth function $h(x,y)$ should approximate $g(x)$ when y approximates zero (0).[2]

The committee function $h(x,y) = (x^2 + y^2)/(x^2 + y^2 + z^2) = P$ was tested using roll call data from the U.S. House of Representatives, 91st Congress, 1st Session. The relevant committee and its membership were ascertained for 145 roll calls.[3] The probability of passing was

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[*] I thank Harvey Arnold, Franklin Burkeen, Harold Casstevens II, Glenn Friedly, Richard Hahn, William Ice, John Lecznar, Roger Marz, Jan Ozanich, and Harvey Smith.

[1] "When they [the Appropriations Committee Members] stick together, you can't lick 'em on the floor." (A remark attributed to an anonymous House leader and quoted by Richard F. Fenno, Jr., *The Power of the Purse: Appropriations Politics in Congress* [Boston: Little, Brown and Company, 1966], p. 37.) "Now is it to lower the price of corn, or isn't it? It is not much matter which we [the Cabinet] say, but mind, we must all say *the same*." (A remark attributed to Lord Melbourne and quoted by Walter Bagehot, *The English Constitution* [London: Oxford University Press, 1961], p. 13n.) This essay adopts Bagehot's view that the British Cabinet is not only *a* committee but also *the* committee of Parliament and especially of the House of Commons.

[2] There is nothing absolute about these desiderata, but they did guide the search for a function to be empirically tested. Our $h(x, y) = g(x)$ when $y = 0$.

[3] *Congressional Roll Call* (Washington: Congressional Quarterly Service, 1970) was used to determine each member's vote on all 177 roll calls and to ascertain the relevant bill for each motion; given the relevant bill, *Digest of Public General Bills* (Washington: Government Printing Office, 1970), 2 volumes, yielded the relevant committee for each of 145 roll calls; given the relevant committee, *Congressional Directory: 91st Congress, 1st Session* (Washington: Government Printing Office, 1969) gave its membership. Motions requiring exceptional majorities were excluded. Changes in committee memberships during the Session were ignored, except when a member vacated his seat in the House (which was noted in *Congressional Roll Call*) and then his committee was considered to have been reduced in size.

Table 1. Preliminary Perspectives on the Committee Function

Proportion of Committee Voting Yea*	Number of Motions*	Proportion of Motions Passed*	Probability of Passing f(x)	Probability of Passing g(x)
0.0–0.1	16	0.0	0.0	0.0 –0.012
0.1–0.2	17	0.0	0.0	0.012–0.059
0.2–0.3	19	0.16	0.0 –0.167	0.059–0.155
0.3–0.4	24	0.21	0.167–0.333	0.155–0.308
0.4–0.5	28	0.43	0.333–0.500	0.308–0.500
0.5–0.6	23	0.56	0.500–0.667	0.500–0.692
0.6–0.7	26	0.89	0.667–0.833	0.692–0.845
0.7–0.8	11	0.91	0.833–1.0	0.845–0.941
0.8–0.9	8	1.0	1.0	0.941–0.988
0.9–1.0	43	1.0	1.0	0.988–1.0

* Donald R. Matthews, *U. S. Senators and Their World* (New York: Vintage Books, n.d.), Figure 22. These data are drawn from the 83rd and 84th Congresses; the lower bound, but not the upper bound (except for 1.0), is included in each proportional category.

computed, using $h(x,y)$, for each of those 145 motions.

A committee-by-committee comparison of the actually observed and theoretically expected results is given in Table 2. Altogether, $120/145 = 0.828$ of the motions passed and the expected value was $115.575/145 = 0.797$. A comparison of the observed and expected values, classified by intervals of $1/10$ in the probability values, is given in Table 3.

The tabular data are quite compatible with the committee function $h(x,y)$. However, given the small number of cases in some cells of the partitions, the goodness of fit may be underestimated in a cursory examination. Fortunately, as arguments such as the following show, the manifest goodness of fit should be taken seriously. (1) Consider the House Committee on Foreign Affairs. The difference between its actual and expected results is the largest such difference in Table 2. The mean probability of passage for the 8 motions is the expected value divided by the total number of motions, to wit, 0.709. Thus, approximately, the probability of 7 motions (the observed number) passing is

$$\binom{8}{7}(0.709)^7(0.291)=0.210.$$

Similarly, the approximate probabilities for the passage of 8 and 6 motions are 0.064 and 0.302, respectively. Therefore, if the committee function generated correct probabilities, there is a probability of approximately $0.210 + 0.064 + 0.302 = 0.576$ of obtaining the observed result ± 1. (2) Consider the probabilities such that $0.2 \leq P < 0.3$. The difference between their actual and expected results is the largest such difference in Table 3. The mean probability of passage for the 7 motions is 0.236. Thus, approximately, the probability of 3 motions (the observed number) passing is 0.155, the probability of 4 motions passing is 0.047, and the probability of 2 motions passing is 0.306. If the basic probabilities are correct, then the prob-

Table 2. Observed and Expected Numbers of Motions to Pass, by Committee

Name of Committee	Total Number of Motions	Actual Number to Pass	Expected Number to Pass
Appropriations	32	27	25.774
Rules	15	15	14.537
Banking & Currency	15	10	10.353
Interstate & Foreign Commerce	11	9	9.374
Education & Labor	10	9	8.393
Ways and Means	9	8	7.692
Foreign Affairs	8	7	5.674
Armed Services	8	6	5.889
Public Works	7	6	5.943
Judiciary	6	4	2.794
Post Office & Civil Service	5	3	2.384
District of Columbia	4	3	2.840
Science & Astronautics	3	3	3.000
House Administration	3	2	2.113
Agriculture	2	1	1.902
Interior & Insular Affairs	2	2	1.900
Merchant Marine & Fisheries	2	2	2.000
Government Operations	1	1	0.966
Joint Atomic Energy	1	1	1.000
Veterans Affairs	1	1	1.000
Total	145	120	115.528

ability is about 0.508 of obtaining the observed value ± 1.

Messrs. Burkeen, Friedly, Hahn, Lecznar, and Ozanich have also tested the committee function by analyzing 223 roll calls (all those for which they could determine relevant committees) in the U.S. Senate, 91st Congress, 1st Session. There were 63 consecutive roll calls related to the tax reform bill; this was an obvious fluke so those 63 were analyzed separately: 31 actually passed and 22.977 were expected to pass. The remaining 160 cases yielded 107 motions which actually passed and 100.386 which were expected to pass. Disturbed by this persistent albeit slight underestimation, Messrs. Lecznar and Ozanich proceeded to analyze 243 roll calls (all those for which they could determine relevant committees) in the U.S. Senate, 90th Congress, 1st Session. They found that 126 motions actually passed and that 135.890 were expected to pass. In sum, 611 roll calls have been analyzed: $384/611 = 0.628$ of the motions actually passed and $374.828/611 = 0.613$ of the motions were expected to pass; if the 63 manifest flukes are omitted, $353/548 = 0.644$ of the motions actually passed and $351.851/548 = 0.642$ of the motions were expected to pass.

The theoretical and practical[4] utility of the committee function h(x,y) is transparent. For a given session of a given legislature, a more exact equation can no doubt be constructed. Indeed, any desired standard of accuracy can be surpassed if a polynomial of any degree is permissible. The simplicity of h(x,y), by contrast, indicates its potential generality as an acceptable empirical law.

When a member of the relevant committee does not vote, the effect is purely to increase the probability of the motion passing. However, in general, the committee function does *not* equate not voting with voting yea.[5] Nevertheless, if $x \leq y$, the committee function does imply that a committee member's switch from nay-saying to nonvoting would increase P at least as much as would his switch from nay-saying to yea-saying.

The effect of a committee member's not voting might intuitively be expected to be symmetrically distributed between increasing the probabilities of a motion's passing and not passing. Specifically, it was initially conjectured that $P = (x^2 + y^2/2)/(x^2 + y^2 + z^2)$. However, if yea-saying is not weighted more than nay-saying,[6] then that intuitive expectation is simply confounded by the preceding data because the computed data err (if at all) on the low side.

In fact, the asymmetrical effect of not voting (by a member of the relevant committee) is actually quite plausible. Legislative folkways prize reciprocity between members; and on motions, this reciprocity seems to be translated as: "Unless the relevant committee says it's a mistake to do so, I'll support any other member's motion."

In summary, the purpose of this note has been to make explicit the previously implicit importance of the committee function as a concept and a problem in the mathematical theory of legislative behavior. The solution to the problem and implicit definition of the concept is $P = (x^2 + y^2)/(x^2 + y^2 + z^2)$, where P is the probability of a motion passing on the floor, x is the proportion of the relevant committee's members who vote yea, z is the proportion who vote nay, and y is the remaining proportion.

Table 3. Observed and Expected Numbers of Motions to Pass, by Intervals of 1/10

Probability of Passing	Total Number of Motions	Actual Number to Pass	Expected Number to Pass
0.0–0.1	9	1	0.436
0.1–0.2	5	0	0.795
0.2–0.3	7	3	1.654
0.3–0.4	5	1	1.859
0.4–0.5	4	2	1.741
0.5–0.6	2	1	1.069
0.6–0.7	3	3	1.982
0.7–0.8	5	5	3.756
0.8–0.9	10	10	8.682
0.9–1.0	95	94	93.602

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[4] For example, if a lobbyist can accurately forecast the committee members' reactions to his desired proposal (and this is a manageable job for a lobbyist), then the lobbyist can derivatively forecast its prospects on the floor (if it is introduced as an amendment).

[5] The pertinent arithmetical fact is that $(x + y)^2/[(x + y)^2 + z^2] \geq (x^2 + y^2)/(x^2 + y^2 + z^2)$, when x, y, and z are non-negative real numbers.

[6] Equal weights for yeas and nays are even more intuitively appealing than a symmetrical impact for nonvoting.

APSA PRESIDENTIAL ADDRESS, 1971

To Nurture a Discipline

ROBERT E. LANE

To Nurture a Discipline

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It is time to take more seriously the nurturance, that is, the support and planned development, of our discipline. The failure to do this implies more current unemployment than need be the case, poorer education than we are capable of providing, a training program incongruent with social needs, an ineffective address to public policy, lost information, and wasted talent. Since men cannot act without concepts to guide their actions, I wish first to set forth some paradigmatic models for our discipline and association. Then, following a brief sketch of the discipline, comparing it to some others, I plan to turn to two specific problems: graduate education and research creativity and productivity.

I. Association Paradigms

I think of university-based professional associations as having three stages of growth and development. First there was the *learned society*, the model that informed the consciences of the founding fathers in 1903 when they defined the purpose of the American Political Science Association as follows: "It shall be the object of this Association to encourage the study of political science. . ." The constitutions of the other social science associations were similar, except that the psychologists, geographers, and statisticians also mention a public service purpose. The principle activities of such learned societies were to publish one or more journals and hold meetings where scholarly papers were delivered and discussed. The National Office was not established until fifty years after the founding of the original learned political science society.[1]

The second model, reflected in the term we use now, is that of the *professional association*. The constitution of the anthropologists, adopted one year before the political scientists', gives the main theme of this model: "to further the professional interest of the American anthropologists." In many ways this reflects the organizing principle of the associations formed by the doctors, lawyers, and engineers, even prior to the formation of discipline-based associations.[2] Guided by the concepts implicit in this model, the social science associations established personnel services, committees on professional standards and ethics; they promoted their interests as these were affected by government and they sought to improve their working conditions, including in the early 'fifties and late 'sixties, the most important working condition, academic freedom. Finally, and belatedly, they sought to improve the conditions of such disadvantaged groups as Blacks, Chicanos, American Indians, and women. These activities, as reported to me by the executive secretaries of the eight main social science associations, were in addition to the learned society activities; it was an enlargement of duties and not a substitution of one set for the other.[3]

Underlying the operating principles of these two concepts were some fundamental beliefs about what might be called the "knowledge process," the process by which the sciences are developed, research conducted, ideas transmitted, men inducted into a training process and then placed in suitable occupations. The most important of these beliefs was the belief in laissez-faire, the satisfactory working of the market for trained specialists, for research information, and for information exchange on educational programs. This belief held that without central information services individuals were able to make intelligent decisions on graduate study and career choices, departments and universities were sufficiently informed to decide on what kinds of programs to offer and what degrees to award, research scholars, perhaps with the aid of a librarian, were able to locate the information they needed for their research, editors and regional or specialized associations were able to decide on what journals were needed and how circulation and business problems were to be met. I am arguing that under present conditions of size, specialization, and complexity the market forces, informal networks, and unaided individual decisions now are wasteful and inefficient.[4] It is time for a new paradigm.

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[1] See Albert Somit and Joseph Tannenhaus, *The Development of Political Science: From Burgess to Behavioralism* (Boston: Allyn Bacon, 1967).

[2] See Corinne L. Gilb, *Hidden Hierarchies: The Professions and Government* (New York: Harper & Row, 1966).

[3] "Social Science and Public Policy: The Role of the Professional Associations," address to the Annual Meeting of the National Research Council, March 22, 1971. A questionnaire to the executive secretaries of eight professional associations forms the basis for some of these remarks.

I will refer to this as *science management* because this is an established term in other areas,[5] but I hope you will understand that by "science" I mean all knowledge, humanistic as well as the more narrowly scientific, and by "management" I mean chiefly the provision of information and facilities by a central service to help guide individual freely made decisions. (I might have called it "knowledge nurturance" but for the awkwardness of the term.) This paradigm embraces several processes designed to help a discipline carry on its business more effectively. First, of course, there is the function of gathering information about the discipline, disseminating it, and analyzing it. That analysis is no longer a simple matter of generalizing from experience, or common sense inferences from raw data. The world has proceeded beyond that point, for along with the substantive development of the disciplines there have developed some meta-disciplines dealing with the organization, dissemination, and utilization of both knowledge and talent.[6] Using the theoretical and empirical knowledge thus developed, we can begin to understand the recruitment and flow of students into the discipline, the more efficient ways of educating and training these students for the purposes we have in mind, the uses of our human resources, and the development of better information exchange systems in the discipline. These processes of analysis represent a form of disciplinary self-consciousness, one that we are just now beginning to employ (a little later than some disciplines—and earlier than others).

The second function involved in science management is a cost-accounting function. The economists have been neglectful of the social costs of the decisions of individual firms, calling them "externalities" and dismissing them. We have done the same thing; the benefits to any individual department of entering upon a Ph.D. training program may be substantial, but the effects on the discipline of these many individual decisions may be harmful. In the same way, it may serve the purposes of a regional association to publish a general journal, but the costs of information loss to the discipline may be very great. Some kind of social, discipline-wide cost-benefit analysis is implied in science management.

Third, there is the ingredient of forward planning, planning for new careers, planning for new archival facilities, planning for information exchange. Just as random unplanned use of land can result in ugliness and waste, so the random unplanned development and utilization of talent can result in personal tragedy and wasted resources. Plans need not and should not be coercive; but if they are well done, they are likely to be persuasive over the long run.

II. Portrait of a Discipline

The characteristics of a discipline help to shape both the agenda and the possibilities of success for a program of disciplinary nurturance of science management. It is surprising how little we know about ourselves. Here are some rapid glimpses into the character of our discipline.[7]

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[4] Political science shares the general characteristics of the growth of science, but in recent years has exceeded even these normal growth rates. See Derek D. Price, *Science Since Babylon* (New Haven: Yale University Press, 1961); for an intelligent critique of this, see Daniel Bell, "The Measurement of Knowledge and Technology," in Wilbert Moore and Eleanor B. Sheldon, eds., *Indicators of Social Change: Concepts & Measurements* (New York: Russell Sage, 1968) pp. 145–245, since Price reports a doubling of scientists and publications about every fifteen years, it is evident that in the past 25 years, at least, political science has exceeded the "normal" rate. For a different view of "the growth of science" see my "The Decline of Politics and Ideology in a Knowledgeable Society," *American Sociological Review*, 31 (1966), 649–62.

[5] Robert Gilpin and Christopher Wright, eds., *Scientists and National Policy Making* (New York: Columbia University Press, 1960); Don K. Price, *Government and Science* (New York: New York University Press, 1954); Gene M. Lyons, *The Uneasy Partnership: Social Science and the Federal Government in the Twentieth Century* (New York: Russell Sage, 1969); *Reviews of National Science Policy: United States* (Paris: Organization for Economic Co-operation and Development, 1968).

[6] There is an ample literature in these two fields of human resources policy and information science. In the first of these the *Journal of Human Resources* is especially valuable. There are two excellent recent studies on education and manpower: John K. Folger, Helen S. Astin, and Alan E. Bayer, *Human Resources and Higher Education: Staff Report of the Commission on Human Resources and Advanced Education* (New York: Russell Sage, 1970), and Dael Wolfle, *The Uses of Talent* (Princeton: Princeton University Press, 1971). The information field has its own professional organization, American Society for Information Science. For political scientists one place to start is with W. J. Paisley, *The Flow of (Behavioral) Science Information: A Review of the Research Literature* (Stanford, Calif.: Institute for Communication Research, 1965); an excellent model for a disciplinary study is provided by W. D. Garvey and B. C. Griffith, *Reports of the American Psychological Association's Project on Scientific Information Exchange in Psychology* (Washington, D.C.: American Psychological Association, 1963–65); see also the report of the Committee on Information in the Behavioral Sciences (David Easton, Chairman), Division of the Behavioral Sciences, NRC: *Communication and Resources in the Behavioral Sciences* (Washington, D.C.: National Academy of Sciences, 1967), and Kenneth Janda, *Information Retrieval: Applications to Political Science* (Indianapolis: Bobbs-Merrill, 1968).

Size. In terms of numbers of professionals, political science is among the smallest of the sciences in the *National Register of Scientific and Technical Personnel* (about a fifth of the size of physics and a quarter of the size of mathematics), but it is a medium sized social science. That is, it is about one quarter the size of psychology, half the size of economics, and about the same size as sociology.[8] It is four times the size of anthropology. Its growth rate (in terms of earned degrees) has been a little slower than psychology and sociology, faster than economics.

Degrees and level of training. In 1970 about 61 per cent of the political science registrants in the National Register had doctorates (an increase of two per cent over two years.) This is a smaller percentage than anthropology and psychology, but a little larger than economics and sociology, and much larger than the natural sciences.

Salaries. In 1970 political scientists with the doctorate received a median salary less than that of any other of 15 disciplines in the *National Scientific Register* except linguistics. This is due to the lower pay they receive in educational institutions; in the Federal government they receive among the two highest salaries (but only a few of the political scientists registered in 1970 worked in the Federal government.)

Work settings. Of the fifteen scientific fields in the *National Register,* a higher proportion of political scientists worked in educational institutions than any other discipline, except for anthropology: three quarters of all political scientists (and 85 per cent of the doctorates) may be found in academia. Fewer political scientists worked in industry and business than any other disciplinary group, again except for anthropology. About 5 per cent worked in the Federal government in 1970 and this was about average for the social sciences. Further, and tragically, this proportion has declined over the past two years.

Type of work. Political scientists think of themselves more as teachers than research scholars. In the *National Register,* a higher proportion of political scientists recorded themselves as primarily teachers than is the case for the natural scientists, and more than the psychologists or economists, but about the same as the sociologists and anthropologists. At the same time, a relatively high proportion of political scientists are in administrative work (whatever the setting)—more than any of the natural sciences and also more than any of the social sciences except economics. For example, compared to all other natural or social sciences, political science has the second highest proportion of faculty serving as deans—and political science deans are the best paid.

Financial support. Of the fifteen disciplines in the NSF roster political science ties for last place in terms of the proportion of members of the discipline receiving federal research or fellowship support. Put another way, in 1969 the total support from the Federal government for the various social sciences including psychology was $129 million; support for political science was by far the smallest amount awarded to any discipline, a little less than $3 million.

Without regard to Federal support, fewer political science graduate students received fellowship, teaching assistantship, or other support than was true of any other social science (1966). The average stipend for political science graduate students was lower than that of any other social science and more of them were, perforce, self-supporting.

The Association. The APSA has the third largest membership of the eight social science (including history and law professors) professional associations and it has grown more rapidly than any of the others in the past decade. On the other hand, financially our Association has increased its budget and resources less rapidly than most other associations, although APSA expenditures in 1970 were second only to psychology. About two-thirds of the political scientists with doctorates in the field are members of the Association, and less than a tenth of those with masters degrees in political science have joined. On the other hand, something like half of the political science graduate students are members—a higher proportion, I believe, than that of any other discipline.

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[7] For the data underlying the following statements, I have relied on many sources. The chief one is *American Science Manpower 1968, a Report of the National Register of Scientific and Technical Personnel* (Washington, D.C.: National Science Foundation, 1969; NSF 69-38) as supplemented by a special report on political science from the 1970 Register, not yet in print, and *Reviews of Data on Science Resources* (NSF 70-50; No. 19, Dec. 1970); also the National Center for Educational Statistics, *Higher Education: Earned Degrees Conferred: (year) Part B—Institutional Data* (Washington, D.C.: Office of Education, various years: OE 5.254:54013-yr.); *Students Enrolled for Advanced Degrees, Fall (year). Part B—Institutional Data* (OE-54019-yr-B); National Research Council, *Summary Report 1970: Doctorate Recipients from United States Universities* (Washington, D.C.: NRC, various years); *Federal Funds for Research, Development and Other Scientific Activities, 1970–71;* Survey of Science Series (Washington, D.C., NSF, 1971); and the BASS report by Neil Smelser and others, *Sociology* (Englewood Cliffs: Prentice-Hall, 1969). I wish to thank my research assistant, Hiroko Shiozaki, for many long hours spent in locating data and calculating statistics.

[8] The criteria employed for inclusion in the Register were as follows: 1968—"A master's degree in political science or 2 years of graduate work with one year of professional experience; or a Ph.D. in political science; or substantial professional achievement in political science; or the equivalent in professional experience. In 1970 the last clause was omitted and the next to the last clause was modified to read ". . . professional achievement in political science as evidenced by contribution to the professional literature." See *PS,* IV (1971), 33.

Enrollments and student body. The number of political science majors has increased over the past ten years more rapidly than most natural sciences and humanities and more rapidly than any of the other social sciences except psychology and sociology. In this sense, undergraduate student interest in our subject is relatively high. On the graduate level, there are more political science enrollees than in any other discipline except history and psychology, and the rates of change suggest that this will not be altered rapidly, even though psychology, anthropology and sociology are increasing their enrollments more rapidly than we are. In the last four years there has been in all fields a declining rate of increase and by now an actual decrease in doctorates in some fields, such as international relations. (See Figure 1 and footnote 29)

As for the characteristics of these graduate students, something we shall refer to later, suffice it to say here that they are intellectually fully as able, by any measure so far employed, as those of other disciplines, with the exception of anthropology. Furthermore, for what it is worth, some studies show that these graduate students compared to those of almost all other disciplines come from homes of higher socio-economic status.[9] At the same time, except for the sociologists, they are more radical[10]—the usual mixture these days.

Morale and divisiveness. There are many issues that divide us (teaching versus research, elite institutions versus others, behavioralism, and so forth); it may be asked whether a discipline so torn and wracked by division and uncertainty can pull itself together to govern itself and, beyond that, to engage in the science management activities I have described. The answer is "yes." The evidence for this is: (1) when we increased the dues for annual members from $15 to a range up to $30, in a depression year, we increased association membership by 3.7 per cent. (2) In 1969 when the American Council on Education asked junior and senior scholars to rate graduate programs, the junior scholars' ratings correlated with senior scholar ratings .98.[11] (3) When the ACE asked over two hundred members of each of several disciplines whether the educational programs they rated were getting better or worse, the ratios of "better" to "worse" in four social science disciplines were as follows: psychology 127, economics 165, sociology 191, and political science 257.[12] Since this is the only comparative study of morale we have, it deserves consideration. (4) When, in 1969, the mixed faculty-graduate student University of Oregon study group asked graduate students at 12 universities whether or not their graduate program was equipping them for the careers they had in mind, 79 per cent said yes and only 12.5 per cent said no.[13] (5) When in 1966 Luttbeg and Kahn asked 56 department chairmen whether the behavioral-traditional split was seriously divisive in their departments, three quarters of them said no.[14] Dahl's "epitaph for a monument to a successful protest" deserves a fresh wreath.[15] And finally (6) given our concern about coalitions and congruent lines of conflict, we should note that "there is little relationship between the divisiveness of faculty in political science departments and the divisiveness of students."[16]

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[9] James A. Davis, *Great Aspirations; The Graduate Plans of America's College Seniors* (Chicago: Aldine, 1964).

[10] Everett Carll Ladd, Jr. and Seymour Martin Lipset, "The Politics of American Political Scientists," *PS*, 4 (1971), 135–44. The inference that graduate students are, next to sociologists, the most radical, rests on the finding that the junior faculty in political science is among the most "radical" group in the social sciences—a little tenuous, but probably valid.

[11] Kenneth D. Roose and Charles J. Andersen, *A Rating of Graduate Programs* (Washington, D.C.: American Council on Education, 1970), p. 34.

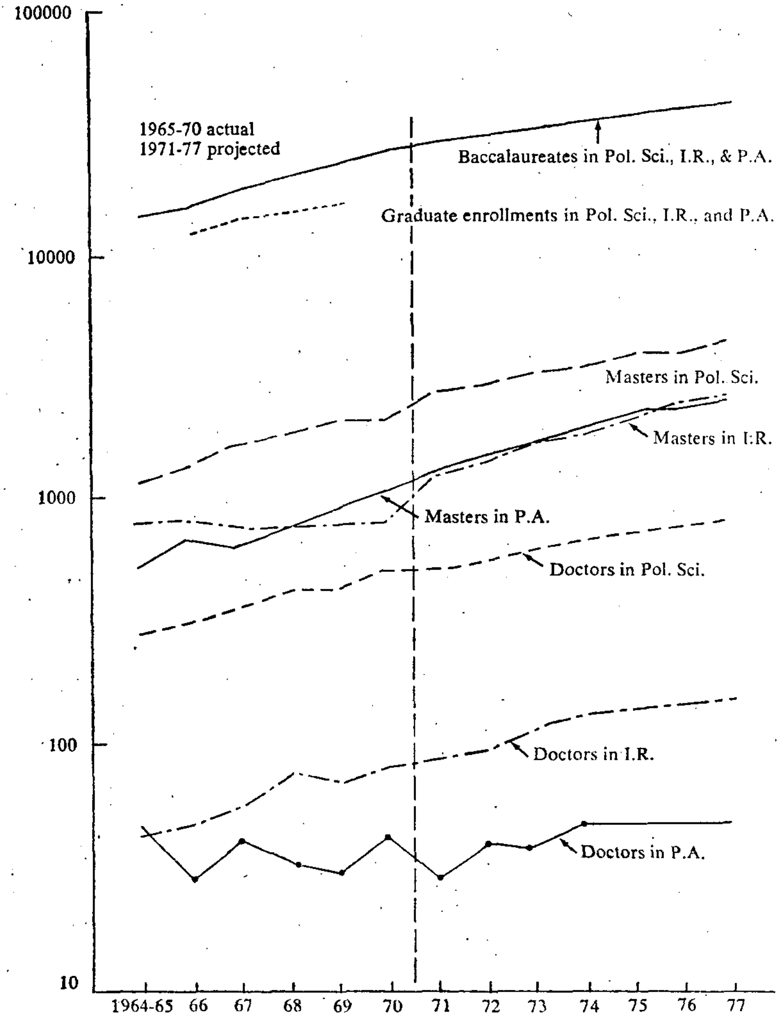
[12] Roose and Andersen. Calculated from reported data for each of the disciplines.

[13] Unpublished report sent to me (8/6/71) by John Orbell and Lawrence C. Pierce, Department of Political Science, University of Oregon.

[14] Norman Luttbeg and Melvin Kahn, *The Making of a Political Scientist: An Empirical Analysis of Ph.D. Programs* (Carbondale, Ill.: Public Affairs Research Bureau of Southern Illinois University, 1969), p. 67.

[15] See Robert A. Dahl, "The Behavioral Approach in Political Science: Epitaph for a Monument to a Successful Protest," American Political Science Review, 55 (1961), 763–72.

[16] John Orbell, Alvin H. Mushkatel, and Lawrence C. Pierce, "The Structure of Professional Education



Sources: see footnote 7.

Figure 1. Earned Degrees in Political Science, International Relations, and Public Administration 1965–77

There is, of course, other evidence of division: junior scholars are more radical than senior scholars and they would accord students more of a voice in departmental affairs.[17] Graduate students are dissatisfied with their training as teachers but not with their training as research scholars.[18] There is widespread dissatisfaction with the "relevance to contemporary problems" of much of our research—shared, incidentally, by students and faculty.[19] And both students and faculty believe, contrary to much evidence, that our enterprise lacks the prestige or status it should have.[20] But on the

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in Departments of Political Science," paper read at the Annual Meeting of the Western Political Science Association, April 1971, p. 17; also unpublished data from the same source.

[17] Ladd and Lipset, "The Politics of American Political Scientists."

[18] "Obstacles to Graduate Education in Political Science," Report of the APSA Committee for an Exploratory Study of Graduate Education in Political Science, *PS*, 2 (1969), 622–41.

[19] "Obstacles to Graduate Education . . . ," and Oregon study cited above in Footnote 16.

[20] In the past, surveys have consistently revealed that college professors rank high in occupational prestige. For example, in Paul K. Hatt and C. C. North, "Prestige Ratings of Occupations," in S. Nosow and W. H. Form, eds., *Man, Work, and Society* (New York: Basic Books, 1962), pp. 277–83, college professors rank 7th out of 90 occupations, well ahead of bankers and lawyers. Nevertheless, as reported in *The Academic Mind* by Paul Lazarsfeld

evidence as I see it, the division within the discipline, our dissatisfaction with our own research, and our sense of lack of public appreciation are not such as to hamper our common effort to improve the quality of the discipline.

The salient features of this discipline, then, are that it is modestly trained (in terms of proportions of Ph.D.'s), is relatively more concerned with teaching and administration than research, is located largely in academia, is relatively poorly paid (except in government), is poorly financed at both faculty and student level, has a professional association with more members than money (but even so fails to include many practitioners of the art), has disproportionately increasing enrollments at the undergraduate level and a levelling off of enrollments at the graduate level, has excellent students, and finally, has a membership divided on many issues but demonstrably capable of common effort.

I have briefly outlined the concept of disciplinary nurturance of science management and I have sketched a portrait of our discipline.

III. Graduate Training in Political Science

The problem for disciplinary nurturance or science management as it belatedly comes to grips with graduate education, is to think in terms of discipline-wide aspects of the field; the individual universities will take care of the teaching and to a large extent the curricular planning. A discipline-wide point of view is different from a departmental point of view in the following respects: (1) it often sees the virtues of the diversity of programs and specializations and the wastes of duplicated efforts, whereas the individual department may try to be like other departments, straining its resources to meet too many objectives within its own confines. (2) To the person with a disciplinary perspective, the mobility of faculty is a more or (often) less efficient method of personnel allocation and of improving individual satisfaction; to the department, mobility means the loss of a valued person (or relief that he is going) and the opportunity or requirement to hire a substitute. (3) The discipline-wide perspective offers possibilities for both special studies of common problems, that are too expensive for a single department, and for the simple exchange of information on curricula, courses, teaching methods, and so forth. At the department level, these matters are viewed idiosyncratically, often personally. In our efforts to understand what we are doing, everything that has been said of the advantages of comparative studies in politics applies to comparative study in education; perhaps even more so. The reason for this special importance has to do with the fact that we are studying ourselves, hence, without alternative models, we are unlikely to have perspective on the possibility or wisdom of change.[21]

The institutional complex. The Association has no committee concerning itself with graduate education, no staff person responsible for collecting and exchanging information about graduate education, indeed, little demonstrated concern, aside from a rather uninformative report ten years ago and a student inquiry two years ago.[22] The costs of this neglect are substantial. The best we can do here is to sketch the general outline of the enterprise and propose some tentative recommendations.

There are about 108 institutions offering doctorates in political science, of which 74 were included in the 1969 American Council on Education survey.[23] Of these, 22 were rated "distinguished" and "strong." These superior institutions produced (1969) about half of the doctorates in political science (18 per cent from the six distinguished universities; 34 per cent from the 16 strong ones.) There were another 22 programs rated "good" or "adequate plus" which in 1969 all together produced another 24 per cent of the doctorates. There then remained in the ACE study 30 doctoral programs generally considered marginal or totally inadequate but included in the report because

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and Wagner Thielens, Jr. (Glencoe: Free Press, 1958), social scientists believe they would be ranked lower than businessmen and lawyers. The low estimate of occupational prestige by graduate students is reported in the APSA "Obstacles to Graduate Education" study cited above: only 32% are even slightly "satisfied with "the prestige of political science today."

[21] For discussion of resistance to change in university faculties, see Theodore Caplow and Reece J. McGee, *The Academic Marketplace* (New York: Basic Books, 1958); Logan Wilson, *The Academic Man* (London and New York: Oxford University Press, 1942); Bernard Berelson, *Graduate Education in the United States* (New York: McGraw-Hill, 1960).

[22] "Political Science as a Discipline: A Statement by the Committee on Standards of Instruction of the American Political Science Association," *American Political Science Review,* 56 (1962), 417–21; *Goals for Political Science: Report of the Committee for the Advancement of Teaching, APSA* (New York: Sloane, 1961); James W. Fesler and others, "Goals for Political Science: A Discussion," this *Review,* 45 (1951), pp. 996–1024. Somit and Tannenhaus's study mentioned above, and an earlier volume by the same authors, *American Political Science: A Profile of a Discipline* (New York: Atherton, 1964). The cited graduate student inquiry is the "Obstacles" report mentioned above. See also Luttbeg and Kahn and John Orbell and others, also cited above.

[23] K. D. Roose and C. J. Anderson, *A Rating of Graduate Programs,* pp. 64–5.

they were located in institutions that had been in the business some time and were producing doctorates. Finally, there are about 34 institutions offering doctorates in political science, not even included in the ACE report because they were too new or too weak, or often both. This complex of 64 inadequate programs, more than half of the total, turned out a quarter of the doctorates in that year—a possible tragedy for the discipline and for the individuals receiving this weak training.

Eleven years ago Berelson said "more students are needed more than more programs;"[24] and, with some modifications about the "more students," this is equally true today. So far as I can tell from published reports, 20 programs (many of them new) had not awarded a single doctorate in the thirteen years up to 1969, while 16 others had awarded from one to three in this period.[25] They offer, but almost no one comes, or at least almost no one finishes. This does no great harm for those who do not enroll, but those who do enroll and don't finish are likely to have wasted their precious years in nonproductive study.

At a guess, I should judge that about 35 new political science doctoral programs have been established in the twelve years up to 1969 or about three a year.[26] New programs are almost always weak ones. For example, of the ten new programs in the ACE study (those in institutions with some modicum of strength) half were judged clearly marginal or inadequate, and only three were judged adequate or better. One of the reasons for this weakness is that it takes time to create both a faculty and a tradition of distinguished research and graduate study, a fact revealed in the clear relationship between a university quality rating (based on ACE data) and the years elapsed since a doctoral degree (in any field) was established: the median years for quality A group is 82 years, quality B is 66, and quality C is 30 years.[27]

On balance it appears that the reasons for establishing a new research-oriented doctoral program are clearly local; the educational requirements of the national discipline do not call for many of these new programs, although some regional needs may modify this judgment. Furthermore, had the Association been alert to the clear warnings eleven years ago[28] (in spite of the recent BASS miscalculations) the current overextension of graduate research training might have been avoided and the cost of our inadequate attention to these problems might have been reduced. Perhaps we have learned a lesson.

But the graduate training enterprise is much larger than the 108 institutions offering doctorates; there are in addition at least 104 institutions offering masters but not doctorate programs in international relations or political science. We will return to these in an Appendix, but taking these together we can estimate graduate enrollment. There are well over 13,000 students registered in advanced degree programs in political science and international relations—next to psychology the largest enrollment in the social sciences.[29] We have no idea of the size of the faculty teaching these students, certainly it is over 3,000, many of whom do not have doctoral degrees.[30] The cost of this enormous enterprise is about $109 million dollars per year.[31] The produce of this effort, aside from *some* valuable training for those who drop out, was in 1969 about 600 doctorates and 3,000 masters degrees (in political science and international relations, but not public administration.)

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[24] B. Berelson, *Graduate Education*, p. 106.

[25] This statistic, by no means easy to come by, is an inference from the fact that none of them was included in the ACE survey (which implies that none of them produced a Ph.D. in political science in the period from 1957–1966) and none of them awarded a degree in 1967, 1968, or 1969 (*Earned Degrees* series.) The data in the next paragraph come from the same source.

[26] Estimated from the above lists and a sampling of past reports on earned doctorates. This compares with a gross increase in the number of *institutions* offering the Ph.D. of 6.5 per year (1957–66) less those discontinuing such degrees, 2 per year; making a net increase of 4.5 per year. See National Science Board, *Graduate Education: Parameters for Public Policy* (Washington, D.C.: NSF, 1969), p. 30.

[27] National Science Board, *Graduate Education*, p. 57.

[28] See Bernard Berelson, *Graduate Education*, pp. 70–80.

[29] These data are from the Office of Education *Enrollment* series mentioned above. The figures for recent years are: 1966 10,438; 1967 11,880; 1968 12,326; 1969 12,862. These are combined political science and international relations enrollment figures. The percentage increases in doctorates for 1967, 1968, 1969 are: political science: 13%, 6%, 5%; international relations 16%, —7%, 0.3%.

[30] The 1968 Register is inadequate for two reasons: (1) political science was included at the last minute and the survey was limited to members of APSA, and (2) there are many more political scientists, judged either by degrees or positions than are members of the APSA. This inadequate data base gives a total of 5,176 political scientists, of which 3,921 were employed by educational institutions. Of this latter group, 2,567 or 65% had doctorates. The 1970 data are better but do not yet provide the detailed breakdown necessary for these calculations.

[31] Estimated by taking the NSB estimate of the total cost of graduate education in 1969 (6.4 billion) and multiplying by the proportion of political science graduate enrollees to total graduate enrollees in 1969 (1.7%) = $108,800,000.

Geographic Distribution and Regionalization. In 1969 the National Science Board recommended that every state and every urban center of over 500,000 population be encouraged and helped to develop a major university. The argument rests on grounds of "equitable distribution of Federal funds for research and development," including access to the benefits of higher education by all segments of the population, "the benefits to society . . . directly realized by the interaction of the institution and the local population," and the influence of a university on the economic development of a region.[32] For political scientists there is another argument: the mutual advantages of close working relations between a state or urban government and a university.

The contrary argument, of course, is expressed in the points made above: we do not need more programs, but better ones; if we need more scientifically trained persons, candidates should be directed to those quality institutions that have excess capacity. Since mobility increases with level of training, there are strong arguments for community colleges, but given the mobility of doctoral candidates, it would be better to bring them to the training institutions than to bring the institutions to them. The regionalization of master's programs has advantages and disadvantages: to the extent that they train for secondary education or local "practice" they might well be regionalized. But on the other hand, they should not be divorced from close association with doctoral training programs.

The regionalization of the markets (output as contrasted to input), for persons trained in some doctoral programs is already a feature of the system—but only for the lower quality institutions.[33] Because of the desirability of heterodoxy and cross-fertilization and because of the current availability of well-qualified persons from national institutions, regionally defined doctoral markets should be discouraged.

Given these considerations, it seems to me that, contrary to the NSB recommendations, our policy should be to encourage the strengthening of existing institutions before new ones are created, at least until a special case is made for a particular place. In 1964–65 the five states with the lowest per capita graduate enrollments were: Texas, Virginia, Georgia, North Carolina, and Ohio. Four metropolitan areas with populations of over 500,000 and no graduate enrollments were: Gary, Norfolk, Tampa, and Youngstown.[34] The argument for new political science doctor's programs in these areas has yet to be made.

The distribution of quality is another, but related, matter. In political science, the South, with 31 per cent of the population has only one third of its share of quality programs (2 out of the top 22 in the ACE 1969 rating). More important for political science nationally is the low quality and high production record of the programs in the Washington, D.C. area. None of the five Washington programs in the ACE report received a rating as high as "adequate plus," yet they produced 165 doctorates in the ten years measured, or 7 per cent of the total. The development of a strong coordinated doctoral program in that area is a matter that should concern both the profession and the faculty of the Washington programs.

Changing quality of graduate education. Concern over the quality of graduate education and the expansion of programs is chronic; for example, in 1934 the Association of American Universities formally resolved that it "views with concern the growth of the number of institutions conferring the Ph.D. degree in fields in which . . . they are not adequately staffed or equipped for work."[35] There are three aspects to the problem: (1) the quality of training at the best universities, (2) the average quality, as affected by the new or inferior programs, and (3) the opportunity cost of those who enroll in inferior programs, combined with the quality of training of those displaced by the products of the new programs.

On the first of these, eleven years ago Berelson said, "as far as the paper standards are concerned, they have gone down," and he cited the loss of the dissertation publication and the language requirements, and what he believed were the less stringent examination requirements.[36] I do not agree, nor does the political science panel (or other social science panels) employed in the ACE report.

(2) The *average* quality of doctoral training may have declined, however, through the entry of new and weak programs. This would occur if the better programs produced decreasing proportions of the doctorates in a field—as they are bound to do if the higher quality programs

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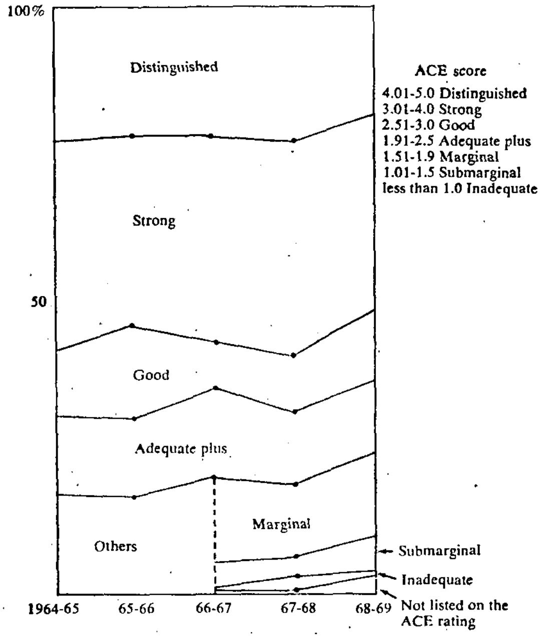
[32] National Science Board, *Graduate Education: Parameters for Public Policy* (Washington, D.C.: NSB, 1969), pp. 35, 40; and NSB, *Toward a Public Policy for Graduate Education in the Sciences* (1969), p. 26.

[33] B. Berelson, *Graduate Education*, p. 118.

[34] NSB, *Graduate Education*, pp. 41, 42.

[35] Quoted in Berelson, *Graduate Education*, p. 31.

[36] Berelson, p. 217.



* ACE, *A Rating of Graduate Programs,* Washington, D.C.: 1970.

Figure 2. Percentage of Ph.D. Degrees in Political Science, By Quality of Program*, 1965–69

do not expand and the new programs attract the students they hope for. At this point the answer is not clear for many of the newer programs are growing and our most recent data are for 1969. As indicated in Table 1 and Figure 2, in 1966 54 per cent of the doctorates in political science were awarded by distinguished and strong departments (as measured in the 1969 ACE rating); in 1967 the percentage was 56 per cent; in 1968 it was 58 per cent; but in 1969 it drops to 52 per cent. Comparatively, this 1969 proportion is about the same as that of the sociologists and historians, higher than that of the psychologists and economists, lower than that of the anthropologists. But Berelson's calm view of this growth process as seen eleven years ago is important to note:

> The production of more second-class doctorates, even though they may lower the average, does not affect the production of first-class doctorates. We shall have as many or more of the latter *plus* the former as well.[37]

(3) If the candidates enrolling in the new or inferior programs would otherwise have gone to better quality programs with excess capacity, there is a net loss in disciplinary quality. If they would not have applied, or if they would not have been admitted if they had applied, *and* if they take the places of persons with training less suited to their jobs (a master's degree in college teaching or policy analysis, for example), there is a net gain in quality of instruction and practice. The average figure is irrelevant. I am reasonably sure that the products of these new programs do take positions that otherwise would have been filled by persons with only the master's degree, but there is no way of knowing whether or not the establishment of new doctoral programs in their own institutions or regions affected their decisions to take a doctorate in political science.

Criteria for the structure and size of departments. In undergraduate education the fewer the students per faculty member the better the education. In graduate education, the National Science Board found that "as the quality of the graduate program [measured by ACE ratings] . . . increases, so does the student-faculty ratio." that is, the better the program the more students per faculty member.[38] On the other hand,

Table 1. Percentage of Ph.D. Degrees in Political Science, by Quality of Program,* 1965–69

Type of Institution		No. in Group	% Total (N=101)	Percentage of Ph.D.'s Awarded				
ACE Score				1964–65	1965–66	1966–67	1967–68	1968–69
4.01–5.0	Distinguished	6	6%	23%	22%	22%	23%	18%
3.01–4.0	Strong	16	16	36	32	34	36	34
2.51–3.0	Good	10	10	11	16	9	10	11
1.91–2.5	Adequate plus	12	12	13	13	14	12	13
1.51–1.9	Marginal	15	15	—	—	15	12	13
1.01–1.5	Submarginal	12	12	—	—	5	5	6
Less than 1.0	Inadequate	3	3	—	—	0.5	2	0.6
Not included in the ACE list		27	29	—	—	0.8	1	3

* Rating according to the 1969 survey of graduate education by the American Council on Education. ACE, *A Rating of Graduate Programs* Washington, D.C.: 1970.

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[37] Berelson, p. 109.

[38] NSB, *Graduate Education,* pp. 93–4.

the Oregon study cited above found that the fewer students per faculty member, providing there were enough students, the better the "organizational climate," that is, the more informal and relaxed were faculty-student relationships.[39] Such an organizational climate is not related to the ACE quality ratings; it may or may not be found in the less distinguished universities. While the relationship between quality of program and high student-faculty ratios is probably an artifact of the greater attractiveness, hence enrollments, in these programs, there is something else which both the Oregon study and the NSB study support. Students learn from each other and a large number of students is important for this process to take place. In the Oregon study, 69 per cent of the graduate student sample were satisfied with student-student teaching, while only 40 per cent were satisfied with the formal programs.[40] For what it is worth (given the artifactual nature of the NSB conclusions about the desirability of a high student-faculty ratio), it may be reported that of the sixteen sciences and social sciences in the NSB study, the distinguished political science departments ranked third in "median graduate student-faculty ratio" (5.88), and even the marginal departments had high ratios.[41] It seems to take fewer political scientists to teach graduate students than it does for the faculties of other disciplines.

Next, there is the question of the minimal size of a faculty that is required before a department should enter into graduate training. The National Science Board finds that the advantages of interaction among scholars, their mutual stimulation, the communications of new ideas, the representation of various approaches to a field, have a joint theoretical basis in the rapid rate of increase in combinatorial possibilities for conversational groups up to seven. Empirically, the NSB finds that of the 16 disciplines in their sample the minimum department size for the distinguished departments is, in fact, seven.[42] But, unless the program is limited to a very narrow range of specialties, perhaps mathematical political science, or, at most, a subfield like international relations, seven is too small for political science. With five major commonly used (although intellectually hard to justify) subfields in this discipline, and with the requirement that each subfield be represented by more than one faculty member, ten is minimal faculty size for doctoral programs in political science. In fact, the smallest of the 22 distinguished and strong departments on which we have data is 12.

Without some idea of the actual size of distinguished and strong departments, this is misleading. In the sixteen disciplines in the National Science Board sample, the median size for "distinguished" departments is 28.5; for "strong" departments it is 20.0. In political science, these median sizes are: "distinguished" about 40; "strong" 34.

Graduate instruction should be done by established research scholars; empirically the ranking of graduate programs declines as the proportion of full professors to the rest of the faculty declines.[43] This is for some obvious reasons, but among others, it is clear that the eminence of a graduate student's sponsor (or dissertation supervisor) is directly related to the student's later achievements, his placements, his creativity. And this, in turn, is because of the crucial importance of an early start along fruitful research lines. Indeed, the prestige of the student's sponsor is more closely related to his later productivity than the quality of the training institution—although, since closeness of the student-sponsor relationship is not a factor, the causal force here may be the selection by bright students of the most imaginative and productive faculty members. But the point is that on the principle of comparative advantage, the more distinguished research professors should spend more time with graduate students and less with undergraduates.

Admissions. In 1903 William James stated that the Ph.D. requirement for university employment meant the recruitment of "men without marked originality or native force . . . the unfit in the academic struggle for existence." In 1909 President Lowell of Harvard warned, "There is a danger of attracting an industrious mediocrity which will become later the teaching force in colleges. . ."[44] These themes, as in so much else having to do with our enterprise, hardly change over the years. The problems are (1) attracting and selecting the best, and (2) developing the talents of students to the fullest extent possible.

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[39] J. Orbell and others, "The Structure of Professional Education," p. 11. These authors find the relationship to be a weak one and they also find that the "organizational climate" is better in the larger universities.

[40] Unpublished data from the Oregon study cited above.

[41] NSB, *Graduate Education,* p. 96.

[42] After the seventh person the increment in combinatorial possibilities reaches its asymptote and levels off; at that point the addition of one more person doubles the number of combinations, but this doubling rate does not increase further (NSB, *Graduate Education,* pp. 97–8). A critique of this theory might point out that the doubling of a good thing, if it works that way and disutilities do not begin to appear (such as anomie), is a good rate of return on any investment.

[43] In sixteen natural and social science fields the institutions characterized by a high proportion of distinguished departments had a median percentage of faculty positions held by full professors of 42 per cent; in the institutions of lowest quality this was 28 per cent (NSB, *Graduate Education,* p. 91).

For political science, attracting and selecting the best requires (a) attracting more persons with mathematical aptitude (our students already have the highest—or second highest—verbal ability scores in the social sciences),[45] and more persons with creativity; and (b) devising better measures for predicting achievement both in and beyond graduate school. The verbal and quantitative ability scores on the GRE, like other intelligence tests, are poorly related to life achievement, including scholarship, and related in curious and idiosyncratic ways (by field and institution) to graduate school achievement.[46] Further, we do not take advantage of measures of creative potential that identify important abilities only partially identified by intelligence type tests. The matter is complex, but since results are field-specific the discipline has an independent stake (separate from that of the graduate deans) in general ability measures as well as the advanced field tests.

Preparation is important: economics majors do as well as political science majors in our graduate work (as judged by advanced placement scores a year or two after admission to graduate school) and a minor in economics is particularly helpful. Within our own discipline specialists in political thought do best.[47] What these have in common, I think, is their training in the use of abstractions.

As for the distinctive qualities needed for teaching and "practice" (work in government), it has long been agreed that we should admit by criteria of intelligence (thus specified and broadened) and screen for other qualities later.

The best development of the pool of talent that is attracted and selected for the discipline is contingent on the strategic use of our combined educational resources. There is every reason to admit "from the top down," maintaining full cohorts at the distinguished and strong universities at the cost of empty seats at the newer and weaker programs. This is not only good for the individuals concerned (the place of training marks a man for life—in that sense the academic class system is a caste system),[48] but it is good for the discipline as a whole, including, in the long run, the newer institutions and colleges that will later employ these new doctorates.

Program content. There is space here for only a few suggestions on doctoral programs: (1) Programs should be more diversified and specialized—we tend to copy each other too much; (2) conceptual analysis is a better preparation for a changing discipline and state of knowledge than is "mastery of its content"—it prepares for the utilization of new knowledge; (3) those who take methods courses do better on content tests[49]—methodology may improve understanding of content; (4) while general social science programs tend to be "thin" and often fail, bilateral interdisciplinary training, as in sociology and law, psychological study of politics, political economy, serve useful purposes—emphasizing depth rather than breadth does not mean staying within disciplinary boundaries; (5) while "professionalization" is discouraged by graduate faculties,[50] economics and psychology offer single programs that serve both future "practitioners" and academic research scholars—we should develop our analytical applied fields, especially policy analysis; (6) our dissertations are the longest of any discipline (in a sample of 22 in 1958)[51] and they should be shortened, with experience given in writing article length pieces; (7) in general, central intelligence on graduate programs, an exchange of information on them, will pay for itself many times over.

III. Creativity and Productivity

It is the function of a discipline to explain and interpret a certain range of phenomena. The nurturance of a discipline means, above all, the improvement of these explanatory capacities.

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[44] Quoted in Berelson, *Graduate Education*, pp. 18, 23.

[45] Special report from the Educational Testing Service on 1970 scores for students taking the advanced test in political science. Also, Philip R. Harvey and Gary L. Marco, *An Analysis of the Graduate Record Examinations Scores by the Undergraduate Major Field of Study, 1963–64* (Princeton, N.J.: ETS, Aug. 1965).

[46] Gerald Lannholm, *Review of Studies Employing GRE Scores in Predicting Success in Graduate Study, 1952–1967;* and G. V. Lannholm, G. L. Marco, and W. B. Schrader, *Cooperative Studies Predicting Graduate School Success* (both Princeton: ETS, 1968).

[47] Special report from ETS; see footnote 45.

[48] See the works of Berelson, Folger et al., and Wolfle cited above; also Diana Crane, "Scientists at Major and Minor Universities: A Study of Productivity and Recognition," *American Sociological Review*, 30 (1965), 699–714. Hiring of one's own doctorates is directly related to quality: in the "top twelve" 47 per cent of the faculty were trained by the university that employed them (1958); in the next ten 27 per cent were inbred; the next group 20 per cent. Berelson, p. 115.

[49] Special report from ETS; see footnote 45.

[50] Berelson, *Graduate Education*, p. 86.

[51] Berelson, p. 181.

A. Creativity. What is creative political or social science?[52] It is formulating an unconventional or novel question, as in Schelling's inquiries into rational strategies for "exploiting potential force," or the turning around of old questions, as the authors of *The Authoritarian Personality* did when they reversed questions on anti-Semitism to ask about the ethnocentric person. It is a novel juxtaposition of things not put together before, as in Herbert Spencer's use of evolutionary theory to "explain" the probable outcomes of political intervention in a system, or as in Deutsch's use of communications theory to explain facets of international relations. It is a perception of a new "figure" or theme against a familiar background, as in Lasswell's discernment of displacement mechanisms in political thinking, or the Supreme Court's finding of a "right to privacy" in the Constitution. It is the discovery of new logical relationships in old materials, as in Arrow's analysis of *Social Choice and Individual Values,* or in Popper's substitution of criteria of falsification for verification of universal propositions. It is in the development of new paradigms for analysis, as in Wiener's development of the ideas of cybernetics, or Almond and associates' restatement of elemental political processes and political crises, or in Daniel Lerner's theory of modernization. It is the invention or novel use of new methodologies, as in L. F. Richardson and Quincy Wright's quantitative and mathematical studies of war, or in McPhee's or Simon's, or Pool, Abelson and Popkin's computer simulation of social and political systems. It is in the definition and elaboration of a new field of study, as in Luther Gulick and Leonard White's development of the field of public administration. It is in the discovery of ordered relationships in a novel way, as in S. M. Lipset's study of the conditions of stable democracy, or in Miller and Stokes's empirical study of representation, or in Riker's studies of political coalitions. It is in the successful use of metaphor and analogy as may be seen in Thurman Arnold's *The Folklore of Capitalism* and Veblen's *Theory of the Leisure Class.*

Deutsch, Platt and Senghaas identify "major advances" in the social sciences with creativity: such advances must help people "to see something not perceived before," or "create the possibility of doing something that had not been done before" (and have a "substantial impact that led to further knowledge").[53] The ingredients of this creativity are the recombination of elements into new wholes or decomposition of wholes into new elements, discernment of new patterns or orders, the reformulation of questions, the invention or novel application of new methods, and above all, the reconceptualization of a problem or field.

As a process, creativity has been said to involve four stages.[54] The first is *preparation,* the learning of the materials and content of a field, a stage which can, if improperly handled, destroy the possibility of creativity by encouraging premature closure and too great deference to authoritative scholars and materials. The second stage is *incubation,* the mulling and puzzling over some problem, sorting things out, tentative search procedures. It is at this stage that openness to fantasy, the promptings of the unconscious, the entry of heterodox ideas, come upon the scene. Almost all studies of creativity emphasize the importance of the unconscious free association process, and some specify that deliberate inattention to a problem, allowing the unconscious further scope, precedes the discovery.[55] Simon argues that unconscious processes are identical to conscious processes, but he is virtually alone in this belief.[56] The third stage is *illumination,* when the germ of the new idea or solution enters consciousness, but, as is often pointed out, the stimulus to the new idea will only be recognized for what it is by a mind prepared for receiving it; Deutsch's familiarity with early cybernetics theories and his knowledge of previous studies of nationalism prepared him for the communications theory of national and international integration.

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[52] Like the study of the use of human resources, information exchange, and education, the study of productivity and creativity has developed a corps of professionals, a body of literature, and a growing corpus of knowledge. Chapter 8 "Determinants of Professional Achievement and Rewards Among Scientists" in the Folger *et al.* book cited above summarizes some of this material. See also: Calvin W. Taylor and Frank Barron, eds., *Scientific Creativity: Its Recognition and Development* (New York: Wiley, 1963); Morris J. Stein and Shirley J. Heinze, *Creativity and the Individual: Summaries of Selected Literature in Psychology and Psychiatry* (New York: Free Press, 1960); John C. Gowan and others, eds., *Creativity: Its Educational Implications* (New York: Wiley, 1967); Howard E. Gruber, ed., *Contemporary Approaches to Creative Thinking* (New York: Atherton, 1962.) Studies emerging from the work of the Institute for Scientific Information employing the Science Citation Index are interesting, especially Jonathan and Stephen Cole, "Measuring the Quality of Sociological Research: Problems in the Use of the Science Citation Index," *American Sociologist,* 6 (1971), 23–29; C. Roger Myers, "Journal Citations in Scientific Eminence in Contemporary Psychology," *American Psychologist,* 25 (1970), 1041–48. I am indebted to Melvin Weinstock of the Institute for Scientific Information for bringing these last two studies to my attention.

[53] Karl W. Deutsch, John Platt, and Dieter Senghaas, "Conditions Favoring Major Advances in Social Science," *Science,* 171 (1971), 450–59. See also critical letters and rejoinder, *Science,* (1971), 1191–92.

[54] Graham Wallas, *The Art of Thought* (London: Cape, 1926). For a brief review of the literature and further discussion of creativity as process, see Ralph J. Hallman, "The Necessary and Sufficient Conditions of Creativity," in J. C. Gowan and others, eds., cited above.

The fourth and more familiar stage is *verification.* Except, perhaps, for the use of novel methods or the invention of indicators and tests, verification is a standard procedure relatively well understood in the behavioral sciences. What is risky here is that the importance of verification be so stressed in graduate school that there is developed in the student an unconscious censorship of novel ideas. Almost all creativity studies mention the need for deferred judgment, for postponing the critical faculties until the new ideas have a chance to emerge. In short, rigor can stifle imagination (but imagination can also discourage rigor).

As a process, creativity can be encouraged and taught by conscious attention to its elements.[57] Students can be encouraged and shown how to avoid too narrow a definition of a problem at the outset, and also how to isolate elements of the problem from the background complexities. They can learn to identify and analyze the properties of these elements (e.g., the functional equivalents of Western "due process" in non-Western societies, the need-satisfying elements in social thinking, etc.). They can be encouraged to develop forced relationships and paradoxical statements for the stimulation of their imaginations; they can be taught to defer judgment and especially to postpone closure on any one solution before canvassing alternatives. The very notion of "idea production" is foreign to most of us, but there is evidence that encouraging the quantity develops better quality, and that later ideas in an idea sequence are better and more original than earlier ones. Indeed, the encouragement to state research as problem solution, rather than as topics for investigation, often helps creativity, hence some attention to anomaly and problemicity will prove useful.

As a discipline we can encourage this rare quality of creativity in several ways. (1) I mentioned the "creation" of the field of public administration; attention might be paid, therefore, to the ways in which new fields are created by *branching,* as political socialization branched off of the field of attitude formation which had previously branched off of the broader field of political parties and public opinion. Branching develops new knowledge partly by intense concentration on a smaller area, but also through the process illustrated by the envelope curve: as the explanatory power of a new set of ideas levels off, those ideas then serve as a platform or culture for another new set of ideas with a thrust of their own.[58]

(2) We can encourage synoptic thinking as a substitute for aggregative thinking, that is, the integration of separate fields of knowledge through the use of new and more abstract concepts, such as "system maintenance" or "interest aggregation."

(3) The conceptual tools for analysis are underemployed in political science. The current apparatus includes game theory, cybernetics, exchange theory, collective choice models, and various kinds of simulation. On the threshold of acceptance are ecological models, mutation models (as in the analysis of language), diffusion models (including epidemiology), and energy-entropy models. These items of intellectual machinery and metaphors have enormous capacities to stimulate the imagination as well as to give precision to analysis.

(4) The integration of concepts themselves awaits exploration: one thinks of the marriage of structural-functional (teleological) models with causal modelling; of developmental and economic theory (Freud used both to good effect, even speaking of "the economy of the mind"), or of the employment of cost-benefit analysis in normative theory, a natural integration since they both deal with the same thing: values given up for values gained.

(5) Creativity is not limited to pure science; it is equally needed and possible in the policy sciences, first in identifying problems and then

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[55] See Sigmund Freud, *On Creativity and the Unconscious, Papers on the Psychology of Art, Literature, Love, Religion,* edited by Benjamin Nelson (New York: Harper, 1958); also reports by creative scientists and mathematicians in Brewster Ghiselin, ed., *The Creative Process* (New York: Mentor, 1955).

[56] Herbert Simon, "Understanding Creativity," in Gowan and others, *Creativity: Its Educational Implications,* pp. 43–52.

[57] See Sidney J. Parnes, "Education and Creativity," in Gowan, pp. 32–43, and especially the seminal piece by J. W. Getzels and P. W. Jackson, "The Highly Intelligent and the Highly Creative Adolescent," in C. W. Taylor and F. Barron, eds., *Scientific Creativity,* pp. 161–72.

[58] See Daniel Bell, "The Measurement of Knowledge and Technology," in Wilbert Moore and Eleanor B. Sheldon, eds., *Indicators of Social Change* (see footnote 4).

in inventing solutions. For example: how shall we develop the moral equivalent of community in a contract society? Or, if freedom of movement concentrates social problems: what selective disincentives to migration should be combined with more direct attacks on the problems at their sites?

Creativity is sometimes studied in terms of the *capacities of individuals,* or as the product of certain personality characteristics. To the extent that these capacities are fixed at the time of student selection, the problem for the discipline is one of selecting the most creative students. One thing is clear: creativity is related to but not the same as the intelligence measured by intelligence tests. Measured separately (through word association tests, imagining various new uses for things, discernment of hidden shapes, making up fables, inventing problems), creativity predicts scholastic achievement as well as intelligence tests do.[59] Moreover it is a relatively general faculty (not subject- or situation-bound). It is associated with social heterodoxy and a sense of humor, and involves a relatively high risk intellectual strategy. In some studies it is seen to be positively associated with dominance, with self-esteem and self-confidence, with the kind of personal security that permits regression and fantasy because an assured return to reality is present,[60] and with the exceptional courage needed to challenge conventional thought as well as some guilt over this challenge process.[61] The important point here is that if it may be reliably measured, such measures should be employed in our graduate student selection. There are obstacles to the selection of creative minds: some studies show that teachers prefer the highly intelligent (high I.Q.) to the highly creative.[62] Thus letters of evaluation may be misleading. But at least we should look for it.

In puzzling over the flowering of creativity in the natural sciences compared to what he thought to be a lower rate of creativity in the social sciences, Wiebe concludes that one important cause for this is the greater discouragement of heterodoxy in the social sciences. To counteract this, he suggests:

> If the social scientists are to increase creativity in their field, they should select their young people with an eye to intellectual audacity, personal courage, and the kind of psychological autonomy that can weather the pressures toward conformity that characterize the fields in which social scientists work. Such people having been selected, their professional training should blend academic rigor with the promotion of dissent, skepticism, unorthodoxy, and audacious innovation.[63]

As indicated in these words of Wiebe's, the environment in which creativity flourishes is both important and subject to some degree of control. Following one line of thought, Maslow's, we might say that the identification of creativity with the process of self-actualization (developing one's potentials), taken together with the belief that self-actualization takes place only after other basic needs, such as safety and "belongingness," are reasonably satisfied, means that some attention to these basic needs is called for.[64] Thus the provision of tenure for faculty and of guaranteed fellowships for a period of time for students helps satisfy the need for safety, while the development of a better organizational climate (warm faculty-student relationships and non-competitiveness) helps satisfy the "belongingness and love needs," both serving to promote creativity.

The earlier argument for encouraging some professionalism (as contrasted to academic emphases) in our graduate programs may, through a tendency to define problems more narrowly, discourage creativity. But given what we said above about the importance of creativity in identifying social problems and inventing solutions, the contradiction might be resolved by seeking, within the confines of professional training, to encourage creative policy solutions. High level business executives come out well on creativity tests.

The kinds of creativity identified by Deutsch, Platt, and Senghaas flourish in metropolitan areas, large universities, and well-financed research centers. More generally we might say that cosmopolitanism and the tolerance of heterodoxy, the achievement of a critical mass of innovative scholars and hence an appropriate on-campus atmosphere of stimulation, and the provision of time and resources are favorable environmental conditions.

Finally, the very lack of coherence in the discipline that we all deplore may imply less pressure towards orthodoxy; the state of knowledge in the discipline is ripe for creative development—perhaps more than in economics or the law.

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[59] J. W. Getzels and P. W. Jackson.

[60] Frank Barron, "Originality in Relation to Personality and Intellect," *Journal of Personality,* 23 (1957), 730–41.

[61] Gerhart D. Wiebe, "An Exploration into the Nature of Creativity," *Public Opinion Quarterly,* 26 (1962), 391–97.

[62] Getzels and Jackson.

[63] G. Wiebe, "An Exploration . . . ," p. 397.

[64] A. H. Maslow, *Motivation and Personality* (New York: Harper, 1954), pp. 80–98.

B. Productivity. Measured by books and articles published, regardless of quality, political scientists are as productive as members of other disciplines and they seem to be more productive than they used to be. In a recent study of the productivity of biologists, psychologists, and political scientists, where the measure of productivity was weighted in favor of continuity of research (thus implying "impact") all three disciplines were equally productive.[65] Two earlier studies of productivity in mathematics and political science (1958) found 46 per cent of the mathematicians but only 25 per cent of the political scientists totally non-productive.[66] In 1960, Berelson found that 33 per cent of the psychologists and 66 per cent of the historians ten years from the degree had published nothing; the comparable figure for political scientists (not quite comparable since the data are for 1968 and the method is a little different) is about 13 per cent. Average publications for this group of ten year doctorates (books and articles) is a little lower for political scientists (3.5) than for psychologists (5.4) but higher than for historians (0.5).[67]

Are political scientists becoming more productive? V. O. Key found in 1958 that 25 per cent of the doctorates awarded twenty years earlier had been totally nonproductive; I find that 17 per cent of the reporting political scientists in the *Biographical Directory* are nonproductive. The findings are noncomparable in many ways, but there is reason to believe that the indicated direction is correct; individually we are more productive of research than we used to be. This, taken together with the increase in doctorates produced each year (about 10 per cent increase per year for the past ten years) has created the information explosion that concerns us all and lies behind the increased *Review* backlog and the increase in the number of journals—at the rate of about five new journals per year. If all political science doctorates produced articles at the rate of those in the *Biographical Directory*, the group of doctorates from one to eight years from their degree would alone have produced about 1,000 articles in 1970–71, enough to fill twenty years of the *Review*. Similarly, they would have produced about 588 books in that one year, almost twice the number that could be reviewed in the *Review*.[68] Fortunately (I guess) these figures overestimate research productivity because (1) there is an attrition in the profession (about 20 per cent in the first five years), and (2) doctorates from the new programs are probably less productive. Nevertheless, something must be done about our abstracting and book reviewing capabilities.

C. Conditions of Productivity. The factors related to productivity are much better understood than those related to creativity; we will discuss them together.

1. *High intelligence.* Creative productivity studies using such sensitive measures as the citation index, reveal that after the minimal intelligence level is reached (one estimate of minimal I.Q. is 122, another is 130) additional intelligence is not related to research productivity—indeed the independent influence disappears when other factors are controlled.[69] Since political science gets its full share of the able and the very able—more than most social sciences—whatever defects our research productivity may have, they do not come, comparatively speaking, because we are stupid.

2. *Motivation.* Speaking of a group of distinguished natural and social scientists, Anne Roe says, "Perhaps the most influential of these other (than intelligence) aspects is motivation—the degree to which the individual's work is important to him. All of these men are, and always have been, so immersed in their work that

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[65] Diana Crane, "Scientists at Major and Minor Universities."

[66] The data on mathematicians are cited in Logan Wilson, *The Academic Man,* p. 107; the data on political scientists are from V. O. Key, "The State of the Discipline," *American Political Science Review,* 52 (1958), 969. V. O. Key goes on to say "The meaning of the data . . . is that we allocate the most inadequate resources to the labor of inquiry essential to the development of the discipline. . . . From my examination of my sample, I would judge, by what seem to me to be latitudinarian standards, that not over 10 per cent had made . . . [significant] contributions." Perhaps this comparison was hard on mathematicians but Folger *et al.* find that their mean citation count is lower than that in other sciences, but higher than psychology (p. 261).

[67] Berelson, *Graduate Education,* pp. 54–55. The figure for political scientists is derived by selecting a random sample of 150 from those with biographical entries (not all political scientists, but most of the doctorates, I think) in the 1968 APSA *Biographical Directory* and counting listed publications. These figures are more reliable for locating nonpublishers than for "average number of publications" due to the shortening of bibliographical entries for older scholars with many entries. The ten-year limit, however, makes this shortening less likely.

[68] Average articles published per person for the first eight years after the doctorate (when articles are still reported in the *Directory*) is .36. In the eight years 1963–70 we produced 3277 doctorates. Multiplying, the result is 1180 articles, reduced to 1,000 because of the lower productivity rate (.28) of those from one to three years from the degree.

[69] Folger, Astin, and Bayer, *Human Resources and Higher Education,* pp. 259–60.

other considerations play much smaller roles."[70] Another study of a large group of scientists found that research motivation, as measured by the time a scientist took to get his degree, had an independent effect on later productivity.[71] Students planning to go into political science at the graduate level are as interested in research as others, but unfortunately, fewer of the very best are so oriented.[72] Further, asked about their primary work activity, the mature political scientists in the scientific manpower register were less inclined to say "research" than was the case in other disciplines.[73] Putting these various studies together, I find the pattern of high research productivity and low motivation puzzling and disturbing. It does not suggest much creativity and raises questions about the quality of the research produced under these circumstances.

3. Research and quality of training. All studies of research productivity, creativity, and reputation stress the importance of the quality of the training institution, where quality is measured either by the American Council on Education ratings or some similar earlier studies. There is an interaction, of course, between the quality of the training institution and the quality of the current work setting, but the relationship to productivity is stronger in the case of quality of training. In Diana Crane's careful study, which included the field of political science, the proportions of highly productive scientists under the four conditions possible with these two variables were:[74]

Educated in a major university and working in a major university	48%
Educated in a major university and working in a minor university	41%
Educated in a minor university and working in a major university (a small group)	36%
Educated in a minor university and working in a minor university	12%

An early study of the productivity of sociologists reveals the same pattern; the later major study by the Commission on Human Resources, working with a very large sample, confirms it.[75]

Under these circumstances, how should one appraise the influence of the new graduate programs on research training and capacity? I have argued above that there are advantages for college teaching to be derived from the new doctorate programs *if* (1) they are additive to (and not substitutive for) the stronger and older programs, and (2) substitutive for the master's degree programs. But as training institutions and work settings for research, the contribution of these programs is different, since there is no substitution effect for the master's programs and there is reason to believe they are only marginally additive to the stronger programs in generating high quality research. I think the summary answer is that these new programs should be regarded as primarily teacher training; their doctorates and faculty may, of course, contribute to quality research, but historical experience does not offer much hope of rapid fulfillment of this aspiration.

4. Research and quality of work setting. The proportion of political scientists working in distinguished or strong departments has been predicted to decline from 34 per cent in 1961 to 29 per cent this year, to 28 per cent in 1976.[76] Yet interaction of these two principle conditions of creative research productivity may have an interesting effect. If Crane is right, then the products of the better universities going to the lesser places will, after a little time—because this syndrome produces delay more than despair—increase the productivity and research orientation of the faculties of the less distinguished universities.[77]

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[70] Anne Roe, "Changes in Scientific Activity with Age," *Science,* 150 (1965), 313.

[71] Folger *et al.,* p. 260; Bernard M. Meltzer, "The Productivity of Social Scientists," *American Journal of Sociology,* 55 (1949–50), p. 28.

[72] Davis, *Great Aspirations,* p. 230. Of those in the top fifth of their classes the proportions in each discipline indicating research interests are: psychology 84%, anthropology and related fields 69%, sociology 56%, economics 55%, and political science 49%.

[73] The percentages of the members of the various disciplines in the 1968 *National Register* who report "research" as their primary work activity are: psychology 26%, sociology 22%, anthropology 21%, economics 21%, and political science 12%. In 1970 this political science percentage dropped to 10.5%.

[74] Diana Crane, "Scientists in Major and Minor Universities," p. 704.

[75] Folger *et al.,* pp. 262–65; B. Meltzer, "The Productivity of Social Scientists," p. 28.

[76] Heinz Eulau and James G. March, eds., *Political Science* (Englewood Cliffs: Prenctice-Hall, 1969.) The increased *number* of programs rated "distinguished" and "strong" should not affect these predictions since the *proportions* of programs with these ratings has remained constant.

[77] D. Crane, "Scientists at Major and Minor Universities," p. 704. I have limited this discussion to universities, but something should be said about colleges as work settings. In Folger *et al., Human Resources and Higher Education,* the ratio of distinguished work in the natural sciences (measured by citation counts) done in universities compared to colleges was 5.85 to 1.0; but in psychology, a hybrid natural-social science, it was only 3.25 to 1.0 (calculated from data on p. 261). In political science, because of its humanistic components, the ratio might be smaller. The matter needs further investigation, but certainly one thinks easily of distinguished political scientists at Swarthmore, Williams, Oberlin, Amherst, and so forth.

5. *Time and resources.* Both the single creative scholar, working with library resources and his own rich store of ideas, and the empirically oriented, data-using work team need as a prime condition of creative productivity the factor of *time*. In speaking of the environmental conditions of creativity, Taylor and Barron say that only one thing is known for certain: "the obvious need for ample time in which to work freely on problems of one's own choice."[78] Time can be "made" and it can be "bought"; if "made" it is squeezed by an act of will out of other duties, and the conflicts, harassments, and guilts that ensue are familiar to all of us. For these reasons bought time is better. The sources of funds to buy time, are, of course, the individual universities and colleges, the private foundations, and the national government, especially, in our case, the National Science Foundation. Consider, then, the cumulative implications of the following:

Almost half of the political science departments in graduate schools have normal teaching loads for junior faculty of more than 10 hours a week: about 70 per cent of the graduate schools have virtually no way in which they can reduce these loads to help a man get over a hump in his research schedule.

Seventy per cent of the political science departments in four year colleges have teaching loads of ten or more hours a week; 84 per cent of them have no way of reducing these loads for research purposes.[79]

Apparently, then, aside from sabbaticals, which are by no means universal, there is little help from the universities and colleges.

As reported above, fewer political scientists (29 per cent) get help from government than any other natural or social science, except linguistics.[80]

Postdoctoral fellowships and traineeships are specifically and consistently correlated with high research productivity. Thus it is disturbing to find that while about 15 per cent of all new doctorates in all fields taken together receive some kind of postdoctoral study grants or fellowships, only 5 per cent of the political scientists do (although this is more than the economists or historians receive).[81]

In political science, bought time is hard to come by.

D. Disciplinary Policy and Research. If we think of research findings and publications as a disciplinary "joint-product," measures to improve the educational, communications, and manpower infrastructure may find more support.

(1) Given what we know about the importance of training in a major university as a condition of future productivity, as stated above the disciplinary policy should be to devise ways of filling training capacity from the top down, with no reduction in admissions by these quality universities, continued and improved free-choice fellowships by the Federal government (now threatening to close down),[82] and informed advice by all counsellors of college students, taking into account the above information.[83]

(2) Providing that there are enough first rate universities to ensure heterogeneity (and there are), the policy of collective inbreeding—but not necessarily inbreeding by each university—should be encouraged. This traditional hiring policy has been somewhat eroded with the growth of the number of quality institutions, but it is still strong: from 1958 to 1965 the proportion of the faculty at the "top twelve" with degrees from any of the top twelve seems to have declined.[84]

(3) The 22 universities in the ACE ranks of "good" and "adequate plus" should avoid collective inbreeding; rather they should recruit from the now plentiful supply of doctorates from the stronger universities. Again this is a traditional policy; in 1958 about 57 per cent of their faculties came from these sources.[85] In this way the current rate of improvement from "good" to "strong" of .6 universities upgraded per year may continue.

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[78] C. W. Taylor and F. Barron, *Scientific Creativity*, p. 373.

[79] Calculated from data in *The Political Science Job Market: Arizona Survey of Hiring Practices and Employment Conditions* (Tucson: Institute of Government Research, University of Arizona, 1970), pp. 57, 59. The survey on which the data are based received useable answers from 163 graduate schools and 461 four year colleges.

[80] 1968 *American Science Manpower*, p. 42.

[81] National Research Council, *Summary Report 1970*, Table 2.

[82] See M. Crawford Young and Robert M. Bock, "Hard Times for the Graduate Schools," *Change Magazine*, 3 (1971), pp. 20–22.

[83] For accounts of the nonprofessional and uninformed criteria employed by candidates for graduate school, see T. Caplow and R. J. McGee, *The Academic Marketplace*, p. 225; B. Berelson, *Graduate Education*, p. 143.

[84] Berelson, p. 115; D. Crane, "The Academic Marketplace Revisited: A Study of Faculty Mobility Using the Cartter Ratings," *American Journal of Sociology*, 75 (1970), 953–64. If the rate of erosion were to continue at one per cent per year, the 1971 figure would be 72 per cent.

[85] Berelson, p. 115.

(4) The evidence that the doctorates of minor universities working in major universities have research records almost as good as any, implies that a constant open search for talent at all levels by the top universities is in their interest as well as in the public interest.

(5) A recent study of the invisible colleges shows that their members (working jointly and severally on a common set of problems) need both the stimulation of ideas from outside their special fields and the assured cumulation of research findings in an identifiable place.[86] In political science there is too much scatteration of articles in too many general journals. Perhaps this is one reason why political scientists are less concerned with continuity, and therefore the cumulative impact of their work, than psychologists or biologists.[87] It may also help explain the high rate of information loss.

(6) Information search instruments should be strengthened. *The International Political Science Abstracts* contains only about 2,000 abstracts per year; the *Sociological Abstracts* contains about 6,000 and the *Psychological Abstracts* about 20,000. Furthermore, journals need more frequent cumulative indexes.

(7) Among the 7,000 political science Ph.D.'s produced in the last 35 years there are more people with research skills than with good research ideas. The study of the invisible colleges mentioned above suggests that these groups are held together more by their members' relations to a few creative scholars than by lateral reciprocal connections. If, as I would argue, the posing of questions and the agenda for research needs more attention than its execution, those who have a special talent for identifying new research areas and fruitful ways of entering them should have their influence magnified in some fashion. It is a central problem for knowledge nurturance.

The terrain covered in this address has been extensive and rocky; there are no prophets on any mountain tops here. Rather, I think, we shall all have to grub amongst the data a little more.

Appendix on Master's Programs[*]

The most neglected aspect of graduate education is the training of candidates for the master's degrees, a neglect that is especially critical because of the size, proliferation, and diversity of the programs. The resources devoted to master's programs are larger than those devoted to doctorates; more faculty members are involved in teaching terminal master's candidates; up until very recently more college students were taught by instructors with master's degrees than by those with doctorates; many high school students are affected by the content and quality of master's programs from which their teachers received their degrees; more master's degrees recipients than Ph.D.'s work for federal, state, and local government; a master's degree in political science qualifies a person for the National Register of Scientific manpower and 38 per cent of political scientists in the Register (1970) have the master's as their highest degree.

Programs. In political science, there are about 100 master's programs in institutions that offer the doctorate and another 100 programs in institutions that do not. The first group is likely to be in much closer touch with the main currents of the discipline and, generally speaking, is of much higher quality. (The master's degree in international relations is likely to be given by a department offering a political science doctorate, hence tends to be of higher quality; generally it represents an interdisciplinary specialization.)

The recently established master's programs in political science tend to be a first (and sometimes final, and sometimes reversible) step into graduate education by departments in undergraduate colleges. During the three year period 1967–69 at least 30 new programs of this kind were established, or about 10 a year. Many of these programs had small enrollments (eight of them had only one student enrolled each), and the completion rate is low (89 enrollees and 11 degrees conferred). Unless a special case can be established (the advantages of a regionalized or of a specialized program), new master's programs in nondoctorate departments should be discouraged.

Students. There are more students in political science master's programs than in similar programs for any other social science except psychology (a much larger discipline); more of them are part time and apparently more of them drop out. On the other hand, those students who enter these programs immediately or soon after leaving high school are more able than those entering master's programs of any other social science. Again we get more than our share of the best, but compared to those going into doctoral programs, master's candidates (that is, those students planning on a terminal master's degree) are markedly poorer (on GRE scores).

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[86] Diana Crane, "Social Structure in a Group of Scientists: A Test of the 'Invisible College' Hypothesis," *American Sociological Review*, 34 (1969), 335–52.

[87] D. Crane, "Scientists at Major and Minor Universities," p. 707.

[*] The data for this Appendix are derived from the same sources as those cited for the doctorates; see especially footnote 7.

Careers. Since more than 90 per cent of the master's degree recipients do not join the APSA, and only a small fraction are recorded in the National Register (about 2,500—less than 10 per cent of the total) we know little about their careers. Of those in the Register, about two-thirds are in educational institutions, but a higher proportion of master's degree recipients than of doctorates work for government. Some years ago, a study showed that about half of the candidates for the master's degree planned to work for the Federal government, while only a quarter planned on college teaching; they may have escaped the Register questionnaire, or their intentions may have changed.

Policy. Eleven years ago Berelson (p. 228) said:

> The Master's degree has lost status in the arts and sciences; has become largely a professional degree, especially in education; is no longer under the control of the top institutions; and cannot be recovered as the acceptable degree for college teachers.

Since that time there have been 21,289 master's degrees awarded in international relations and political science, many of whose recipients are teaching in four and two year colleges. Part of the reason for this has been the shortage of doctorates, but that shortage has now changed. Further, enrollment in master's programs has levelled off, while doctorates have continued to increase, with the result that the proportion of doctorates to masters has been increasing slowly but consistently: in 1966 it was about 1 to 6 while four years later it was 1 to 4.5. We are now in a position to try to enforce Berelson's dictum that the master's degree is not "the acceptable degree for college teachers."

What, then, are master's programs good for? First, they serve to upgrade high school instruction and insofar as they are teaching degrees they should be so designed. Second, they permit special interdisciplinary teaching programs for international relations, public administration, urban studies, and so forth. These degrees are then certificates of specialized study, partly for practice, and partly for teaching in specialized programs, perhaps on the way to or in addition to a doctorate in a basic discipline. Third, they are excellent vehicles for teaching practitioners (professionals), upgrading skills, and, incidentally, are the basis for salary increases. Fourth, as reported above, without some special strength (as in the case of Swarthmore, for example), and special local reasons (as in the case of an urban college teaching urban politics), departments not offering the doctorate should not offer the master's degree in political science. Finally, in order to keep recipients of master's degrees in touch with the discipline, it might be useful to establish a special class of Association membership at reduced rates whose members would receive a book review journal and *PS*.

COMMUNICATIONS

To the Editor:

In a recent letter to the editor (*American Political Science Review* [September, 1971] p. 779), Lawrence Mayer asserts that my argument concerning the Casstevens result (*American Political Science Review* [March, 1971] pp. 188–189) "contains a mathematical error," and attempts to demonstrate this by trying to show my definition of a certain conditional probability to be inequivalent to an alternative definition. However, the error is Mayer's, not mine, and lies in the fact that he defines the quantity n_i as "the number of votes received by candidate C_i" (p. 779, left hand column lines –18, –17) and then implicitly equates this to the quantity denoted by the same symbol in my letter. In fact, I explicitly defined n_i as "the number of votes received by candidate i from all voters *except the one in question*" (p. 188, left hand column, lines –3, –2; italics added). In Mayer's example the first voter is the one in question, so *the* n_i *should refer only to the* votes cast by voters R_2 and R_3. Mr. Mayer informs us that he is a mathematician, so presumably he will be able to verify for himself the fact that when the proper definition of n_i is used, his "simple counting argument" shows the two definitions of the conditional probability $P(W_1/V_1)$ to yield identical results, as of course they must.

Casstevens himself, in a private communication, has criticized my argument on other grounds. He objects to my use of the action V_0, citing the passage in his note which reads "The alternative courses of action available to the voter are to vote for C_1, or to vote for C_2, . . . , or to vote for C_n. (The problem is how the voter should vote rather than whether or not he should vote.)" (*American Political Science Review* [March, 1968], p. 205.) The difficulty lies in the fact that Casstevens was not explicit about definitions or assumptions in his note; thus it is not clear, for example, whether one should conclude from the quoted passage that he simply failed to take account of the possibility of nonvoting (irrespective of purpose of the analysis), or rather that he explicitly assumed that action not to exist. The more fundamental question concerns what constitutes a fair reading of the even less explicit statements about "ignoring ties"—apparently primarily on the grounds that they are improbable occurrences (footnote 4, page 205)—as they apply to such potential tie-producing events as $n_1 = n_2 > n_3$, $n_1 = n_2 + 1 = n_3$, $n_1 = n_2 = n_3$, etc. I personally think it not unfair to interpret Casstevens's statements about ties to mean that he meant to assign zero probability to such events.

Probably Casstevens himself would not agree, and possibly he might even maintain that had he been asked some three years ago to clarify his assumptions on ties, he would have responded that what he really had in mind was to require the last event to have strictly positive probability, while simultaneously requiring events of the first two kinds to have zero probability (this being the only way the assumptions can be made consistent). I have some difficulty accepting this myself, but for purposes of argument suppose we grant the point and consider just what his result does then say. Many have long believed that under the plurality system of election, it can sometimes be counterproductive to vote for one's most-preferred candidate. No one, so far as I know, has claimed it is *always* counterproductive to vote in this fashion: for example, a voter whose own candidate has the best chance of winning will not find it counterproductive to vote "sincerely," nor will one who believes all candidates have an equal chance of winning. Casstevens' result, properly interpreted, in effect amounts to showing that there is still another species of voter who does not find it counterproductive to vote sincerely. It is a bit difficult to characterize this type of voter explicitly, but he is one who believes, among other things, that there is absolutely no chance whatever of any kind of tie if he does himself vote, while simultaneously believing that should he not vote, there would be a strictly positive probability of an exact three-way tie between all candidates (in the three-candidate case). Perhaps this fact is a useful result, or perhaps it is just a curiosity; but surely, it does not constitute a proof that a rational voter always votes for the candidate he most prefers.

Gerald H. Kramer

Yale University

To the Editor:

I was much interested by the conclusions reached by David Nexon in his article "Asymmetry in the Political System: Occasional Activists in the Republican and Democratic Parties, 1956–1964," *American Political Science Review* 65 (Sept., 1971) 716–730. I think that it is fairly obvious that the conclusion that "the Republican Party organization is essentially an ideological one relying on ideological incentives to activism, while the Democratic Party Organization is a nonideological one relying on nonideological incentives to activism" (page 721) is not warranted by the facts that Nexon presents. To establish this would require data on incentives and motives for participation which Nexon does not have. He appears to have been engaging in some wild speculation. However, his thoughts are intriguing and might be researched further.

More relevant to the data examined is the conclusion that: "On issues that polarized the mass membership of the two parties, Democratic activists had about the same opinion distribution as rank and file Democrats, while Republican activists were substantially more conservative than their rank and file membership" (page 720). This is, as Nexon notes, the same conclusion reached by McClosky *et. al.* back in 1960. McClosky found, however, that Democratic leaders too were significantly different from their rank and file. McClosky's Democrats were closer in some assumed spatial model to both their own voters and the Republican voters than were the Republican leaders. In any case, both articles purport to show that Republican gladiators are the "odd men out" ideologically.

Distances on spatial models of ideology cannot be legitimately determined from the kinds of questions asked by McClosky and Nexon. The averaging of individual scores used by McClosky and the mean scores used by Nexon do not permit the differentiation between the unity of opinions within a group on the one hand, and the content of those opinions on the other. The critical factor here is not the mean or the average, but the distribution. The group of Democratic workers may not be a unified group of moderate men, but rather a group composed of a large gang of "extreme conservatives" and another large bloc of "extreme liberals." Such a group would have a mean score which would be "moderate" (i.e., in the middle), but the conclusion that it is similar to some other group which has a middle-range score may or may not be valid.

Much the same thing applies to the Republican groups. The fact that their scores deviate from the scores of the other groups may not be because their solutions to public problems are very dissimilar to those of the other groups. It may just be that the Republicans are a fairly unified group of individuals who (to use Converse's terms) share a common belief system which has a great deal of constraint. The Democrats and the groups of voters, on the other hand, are much more divided on issues.

This conclusion is particularly likely given Nexon's observation that: ". . . a disproportionate number of Democratic party identifiers have, at least since 1860, come from the South" (page 722).

Issues and opinions are very complex matters. To really get at these questions requires more in-depth questioning than used in the interviews utilized by either McClosky or Nexon. And judgments on whether a particular proposed government policy is relatively close or far away from another proposal are rather difficult to make. It does not suffice for Nexon to use questions about "how strongly" a person agrees or disagrees with a certain statement or policy. A *strong* disagreement does not necessarily mean that the respondent would ideally prefer a policy which drastically differs from that given in the statement, just that he is very much committed to *something* different.

In making these changes in Nexon's conclusions, it may be that I have justified his assertion that the Republicans are more "ideological" than the Democrats. For what Nexon and McClosky have actually shown is that Republicans share a common belief system, and Democrats do not. In a certain sense this means that Republicans are ideological and Democrats are not. It does not, however, show that the members of either party are motivated by ideology, nor does it show that either party's policies are more or less like those of the voting public. The Democrats do resemble the voting public in that both groups are internally divided on public issues.

THEODORE S. ARRINGTON
University of Arizona

TO THE EDITOR:

Professor Arrington criticizes the conclusions reached in my article on a number of grounds. First, he argues that my statement regarding the differing basis of organization in the two parties is not justified by the data presented. It is true that direct data on the "incentives and motives for participation" of activists would have been more conclusive evidence for my finding. Since such data were not available, I relied on the data I had. It is possible to classify the process by which I progressed from the available data to the conclusion noted above as "engaging in some wild speculation." Understandably, I prefer to regard the process as reasonable inference. The logical basis for that inference is discussed at some length in the article itself and reiterated briefly in the last paragraph below.

I find Arrington's second point rather confusingly stated. At one point he seems to be arguing that it is invalid to say that Republican activists are more conservative than other groups because questions of the type used by the Michigan Survey Research Center do not capture opinions and issues in their full richness and complexity. At another point, he seems to be arguing the more restricted point that one cannot say that two distributions of opinion are alike (or different) simply from comparison of mean scores.

The latter point certainly has some validity in a mathematical sense, at least insofar as two similar means are involved. A symmetrical bimodal distribution can certainly have the same mean as a unimodal distribution. Parenthetically, I might note that the "true" shape of a curve is rather moot in any opinion data, given the absence of interval scales. I did compare the shape of the curves of the groups discussed in my article by dividing the sample into quartiles of liberalism on each of the opinion scales and then looking at what proportion of each group fell into each quartile. This procedure produced the same result as looking only at means. The tables of this type were not presented in my article, because, given the number of groups simultaneously compared, I thought the mean was a more readily comprehensible statistic and because the number of activists, particularly among the Democrats, was so small that dividing the cases among four cells was of doubtful statistical validity.

Arrington's belief that the kinds of questions used by the Survey Research Center do not "really get at" opinion is a valid one for certain kinds of theoretical interests. One could hardly hope to develop the findings presented in Robert Lane's *Political Ideology* from reliance on the SRC's national sample alone. One the other hand, the questions used in my scale are fully adequate for indicating, for example, that Republican activists as a group are more likely to favor the nomination of a candidate like Barry Goldwater than are ordinary Republicans, ordinary Democrats, or Democratic activists. Conservatism in this sense has both a theoretical and a practical interest for most political scientists.

Finally, Arrington discusses possible meanings of ideology that may be legitimately regarded as applicable to the Republican activists in my article. The meaning of ideology that I regarded as most important was drawn from the distinction between amateur and professional made by James Q. Wilson and noted in my article. The ideologue in this sense is the person who participates in politics because of his opinions about public policy and not for some other motive. I believed that Republican activists could legitimately be characterized as ideologues in this sense because they were quite distinct, in opinion terms, from other Republicans. This difference between activists and ordinary members must have some causal roots. Controlling for demographic variables did not, with the ambiguities noted in the article, eliminate the difference. I suggested that a plausible and reasonable causal explanation of the difference might be that the Republican Party, unlike the Democratic Party, relies on ideological incentives to activism and thus has a tendency to draw the most conservative of its identifiers into activism. In default of a more plausible hypothesis or proof to the contrary, I hold to that explanation.

DAVID NEXON
Pitzer College

TO THE EDITOR:

In the *American Political Science Review* (September, 1970, p. 979), you published a review of my book, *Ghana's Foreign Policy, 1957–1966: Diplomacy, Ideology and the New State,* by Kofi Ankomah. In a subsequent issue, Professor William Zartman criticized Ankomah's review of my book on a number of grounds. With the same sort of innuendo and *ad hominem* arguments used in his original review, Mr. Ankomah has now cast aspersions on Professor Zartman, by noting that I thanked him in the preface of my book for his helpful suggestions on the manuscript.

Professor Zartman undertook the reading of my manuscript for Princeton University Press in the autumn of 1967. I had not met him at that time. I subsequently came to know him through the intermediary of Princeton University Press. His comments were no less helpful for the fact that they were done for the Press. This does mean, however, that Ankomah owes Professor Zartman an apology.

W. SCOTT THOMPSON
Associate Professor of International Politics
Fletcher School of Law and Diplomacy

TO THE EDITOR:

We write to correct three errors of interpretation which mar Kenneth Shepsle's otherwise balanced review of our proposals for *The Analysis of Political Cleavages* (this *Review*, September, 1971). We have no objection to his quite appropriate overall judgement—a skeptical admiration informed by a sense of the problem's enormity. But on three main points of substance, he has misread or misunderstood the book, leaving himself and his reader with a mistaken impression. The rubs are these:

1. Pairwise analysis of cleavage relations: Shepsle says we make a ". . . unique and imaginative (if not altogether justified) *sociological assumption,* namely that the relevant theoretical focus is not on individuals *per se* but on *pairs* of individuals." We have italicized "sociological assumption" to draw attention to its inaccuracy. We make no such claim, as the reasoning behind the measure being discussed makes quite clear. Shepsle quotes us on the point by substituting his word "is" for our words "may be" and by omitting the beginning of the pertinent sentence, "For these reasons . . ." This bespeaks his neglect of the reasons themselves, which are given in the passage just before he quotes us. A look at these suggests that we have made no sociological assumption—no assertion that politics, like "love and tennis," is "played in pairs." We have just, in fact, been discussing a committee of *ten* members, and have followed that example with the suggestion that relations of sameness and difference in such groups can usefully be exfoliated from the examination of pairwise comparisons (pp. 26–27). In the passage omitted, we suggest that the pair is a useful *analytic* unit on two grounds: 1. it is the smallest unit for which similarity and difference (say, agreement and disagreement) have any meaning, and 2. these relations (similarity and difference) in larger groupings can be analytically reduced to the outcomes of these pairwise comparisons. The review argues against an approach we simply haven't considered.

2. Definition of groups: The analysis we propose assumes that one can figure out what cleavages—which divisions of a community into nominal groups—matter. The book illustrates a serious problem with a rather fatuous example—the silliness of the supposition that India is religiously homogeneous because the classification Christian-Jew-other indicates little fragmentation (p. 34). Admitting that the authors are "aware" of such a possibility, Shepsle transposes the example to Ulster and gives it as his own. He then suggests that we offer "no theoretical guidance" on the matter. He has here omitted two considerable passages of the book, one outlining the underlying assumption about nominal groups (pp. 14–17), and the other offering two specific strategies for dealing with the central problem (pp. 34–35). Here, Shepsle notwithstanding, we *do* depend on a brittle sociological assumption, namely, that well-defined nominal groups are there to be found. This is the point we make and it is a point which makes us cautious about any indiscriminate application of the analysis.

3. Cross-cutting and fragmentation: Shepsle credits us with the "carefully derived" result that ". . . the amount of cross-cutting between two cleavages (and hence the likelihood of political stability) is high if fragmentation is high on one of the cleavages and low on the other." This is a two-fold misreading: 1. *we* claim to have demonstrated nothing about political stability, and 2. far from being a carefully derived result, the claim about cross-cutting and fragmentation is denied by the book itself. We do say that the combination of low and high fragmentation is a necessary condition to high cross-cutting, but deny that it is sufficient. We go on to show that the functional relationship between these variables upholds this denial (pp. 98–99.) Since his proposed counter-example assumes a necessary and sufficient relationship, we can only guess that he failed to read the whole of the (difficult, perhaps turgid) chapter in question. It is, however, only by means of *both* misapprehensions that Shepsle is able to have us predict no conflict over language in Belgium. The claim is, if anyone's, Shepsle's.

The review's fourth main point, on our failure to resolve the intensity problem, is quite right as the book's preface suggests. And, as we indicated at the beginning, the overall judgement expressed by the review is unobjectionable.

DOUGLAS W. RAE
MICHAEL TAYLOR

TO THE EDITOR:

The book review editor has invited me to respond to the Rae-Taylor critique of my review of their book and has requested brevity. Therefore, in response I should like to make two points. First, regarding the second and third "rubs" Rae and Taylor outline above, the thrust of my review was to underscore a serious omission in their monograph. In the absence of any indication of the *salience* of cleavages, their assumption that "well-defined nominal groups are there to be found" is indeed "brittle." In order to analyze political cleavages one must be able to identify the subset of partitions of the populations into nominal groups, i.e., the subset of cleavages, that is *politically relevant.* The assumption that such cleavages can be identified—Rae and Taylor's assumption—may be a reasonable research strategy, but it does beg an otherwise interesting and important question, namely, why do some cleavages manifest themselves in political conflict whereas others do not? On this latter question Messrs. Rae and

Taylor may be referred to the work of the late E. E. Schattschneider.

The cross-cutting cleavages hypothesis, of course, is subject to precisely this kind of criticism. Thus, the Rae-Taylor analysis of it suffers in the absence of a measure of cleavage salience. My own example of Belgium was employed to underscore this omission.

The first "rub" to which Rae and Taylor refer is mostly a matter of semantics. If they are embarrassed by my attribution of sociological properties to their assumption, and wish to call it something else, that is their prerogative. In any event I am quite willing to accept their pairwise treatment of cleavage relations as an *analytic* device. However, if one wishes to judge the utility of that device he must look to its consequences; and it is this operation that is frustrated by errors of omission and commission that crop up later in Rae and Taylor's analysis (see point 4 in their letter above, as well as my discussion of salience, for examples of this).

To conclude, my review of Rae and Taylor's monograph had one summary theme—a theme applicable perhaps to many analyses of complex topics. That theme, I feel, still applies: *caveat emptor*. To this, at least, they seem to have no objection.

KENNETH A. SHEPSLE
Washington University

EDITORIAL COMMENT

Hello again. There are two hypotheses capable of explaining the absence from the *Review* this last half-decade of articles by Austin Ranney. The first hypothesis is that the tasks of managing the *Review* were so onerous as to create a pause in the productivity of one of the most active and interesting scholars in our profession. The appearance in recent years of an occasional article by Ranney in one or another of the regional journals does not wholly undermine this hypothesis, but weakens it sufficiently to suggest another, namely that Ranney thought it unseemly for the manager of a complex process of anonymous evaluations to give his own work what could be interpreted as a natural advantage.

It is therefore likely that the only benefit readers of the *Review* shall realize from the retirement of Austin Ranney as managing editor is his welcome reappearance in these pages as an author in the current and, one hopes, in many a future issue.

Two hypotheses are now available to explain the mysterious absence from the pages of the *Review* of articles by the current managing editor.

APSR and PS. A few years ago, the Association established a newsletter, *PS*. Part of the job of this new journal was to relieve the *Review* of some of the heavy burden that it used to carry as the sole medium of general communication among Association members. The *Review* has continued to provide articles and reviews of scholarly and scientific interest, while *PS*, especially under the innovating leadership of Professor James David Barber, has carved out another sort of niche for itself, providing information about teaching and the profession, notices of career changes and opportunities, official documents of the Association, and other such material.

As time has gone on, the activities of the profession itself have come in for occasional scholarly investigation. Since the *Review* is devoted to scholarship and *PS* to the profession, which journal should receive an article that seems to fall neatly between them? We have, in general, resolved the issue in favor of *PS*. Both journals have the same audience, of course, since both go to all members of the Association as a perquisite of membership. We take for our province contributions that are in some sense "about" politics, and refer to our sister journal articles that are "about" political science as a profession.

The one great anomaly in this orderly scheme of things is presented by the problem of the annual address of the outgoing APSA President. There is something to be said for preserving continuity in making the Presidential address available to readers in the same place, year after year. In the past, this place has been the lead article in the December or March issue of the *Review*. Yet carrying on the tradition creates its own asymmetries: the Presidential address is the only unrefereed article the *Review* prints. Some comments, rejoinders, communications, and book reviews are solicited, and most are normally evaluated only by the editor or book review editor. Hence these contributions are scrutinized less elaborately and less anonymously than articles. The fact still remains that the Presidential address alone out of the entire yearly output of the *Review* passes through no editorial screen at all. Moreover, no Presidential address has been stopped at the gate since Frederic Ogg refused to print President Benjamin Shambaugh's address in 1930, and none is likely to be again, not even by the most nostalgic managing editor. Some Presidential addresses however, perhaps most, violate a basic rule for inclusion in the *Review;* they are not "about" politics, but "about" political science, and by our normal criteria, now that *PS* is flourishing, such essays should logically appear in *PS*. If they do not, does this not gratuitously appropriate to the *Review* a little extra cachet and imply that *PS* is somehow number two? Would not the Association and the members be better off if, instead of living with this implied rank-order, each journal cultivated its own garden on a more strictly equal basis? If *PS* had existed in 1958, "The State of the Discipline," by V. O. Key, Jr., would presumably have been comfortably at home in its pages, whereas Robert A. Dahl's 1967 address, "The City in the Future of Democracy," to take a contrary example, would have been more suited to the *Review*. What about cases occupying something nearer middle ground, like Harold Lasswell's "The Political Science of Science" (1956) or E. E. Schattschneider's "Intensity, Visibility, Direction and Scope" (1957)? Logicians, hair-splitters, proponents of one solution or another, or one or another method of finding a solution (e.g., random assignment, Indian wrestling between the editors of *PS* and the *APSR*, a mail ballot of the Association, President's choice) could have had a field day with the problem. Heaven knows, political science provides little enough recreation as it is, and so one almost regrets the past absence of the opportunity for this sort of exercise.

In future we may be more fortunate. The most likely outcome, however, in the absence of a strong Presidential initiative is that inertia, not logic, will prevail, and the *Review* will continue to steal this small advantage from *PS*, and print Presidential addresses, no matter how preoccupied with the profession they are. As a sop to our troubled consciences we will probably channel responses and reactions to Presidential addresses that "really" belong in *PS* into the *PS* editorial process, thus providing an inelegant but one hopes an intelligible solution.

Relevance. The *Review* is always in the market for articles that meet some standard of contemporary relevance. Although no reader who contemplates our year-long backlog will doubt that we publish first and foremost for the ages, there is a set of possible contributions—not wholly hypothetical—that speaks to contemporaries as well as to the next generation of graduate students, and still maintains a level of empirical demonstration or theoretical interest that makes it eligible for inclusion in the *Review*. Consider four such articles in the current issue, all on currently "hot" topics. One explores the forces at work in Britain surrounding its recently negotiated entry into the Common Market. One analyzes Congress and the Defense Budget. Two are on various aspects of Presidential elections: Austin Ranney casts a doubt upon the political justification underlying the drive for more and more Presidential primary elections, and Robert Axelrod marshals data that can aid in the task of determining how much credence to put in perennial claims about the reformulation of the major parties' electoral coalitions.

Obviously, this does not exhaust the ways in which political scientists can be "relevant," but it is a fair sample. We hope to be able to offer other examples as time goes on.

Articles Accepted for Future Publication

Paul Abramson, Michigan State University, "Intergenerational Social Mobility and Partisan Choice"

Richard W. Boyd, Wesleyan University, "Popular Control of Public Policy: A Normal Vote Analysis of the 1968 Election"

David W. Brady, Kansas State University, "A Research Note on the Impact of Inter-Party Competition on Congressional Voting in a Competitive Era"

Charles S. Bullock, III, University of Georgia, "Freshman Committee Assignments and Reelection in the U.S. House of Representatives" and "House Careerists: Changing Patterns of Longevity and Attrition"

Charles E. Butterworth, University of Maryland, "Averroës: Politics and Opinion"

William D. Coplin and J. Martin Rochester, Syracuse University, "The Permanent Court of International Justice, the International Court of Justice, the League of Nations, and the United Nations: A Comparative Empirical Survey"

Robert S. Erikson, Florida State University, "Malapportionment, Gerrymandering, and Party Fortunes in Congressional Elections"

John Ferejohn, University of Illinois, "A Note on 'Voting or a Price System in a Competitive Market Structure' "

Alexander L. George, Stanford University, "The Case for Multiple Advocacy in Making Foreign Policy"

Wallace Mendelson, University of Texas, "From Warren to Burger: The Rise and Decline of Substantive Equal Protection"

Eugene F. Miller, University of Georgia, "Positivism, Historicism, and Political Inquiry"

Andre Modigliani, Harvard University, "Hawks and Doves, Isolationism and Political Distrust: An Analysis of Public Opinion on Military Policy"

Sarah McCally Morehouse, Manhattanville College, "The State Political Party and the Policy Making Process"

Donald G. Morrison and H. Michael Stevenson, York University, "Integration and Instability: Patterns of African Political Development"

Edward N. Muller, State University of New York, Stony Brook, "A Test of a Partial Theory of Potential for Political Violence"

Willard A. Mullins, Carleton University, "On the Concept of Ideology in Political Science"

John M. Orbell, University of Oregon and Toru Uno, San Francisco State College, "A Theory of Neighborhood Problem Solving: Political Action *vs.* Resident Mobility"

Benjamin I. Page, Dartmouth College and Richard A. Brody, Stanford University, "Policy Voting and the Electoral Process: The Vietnam War Issue"

Gerald M. Pomper, Rutgers University, "From Confusion to Clarity: Issues and American Voters, 1956–1968"

Jeffrey L. Pressman, Dartmouth College, "Preconditions of Mayoral Leadership"

George F. Quester, Cornell University, "Some Conceptual Problems on Nuclear Proliferation"

Bruce W. Robeck, Texas A&M University, "Legislative Partisanship, Constituency and Malapportionment"

Thomas W. Robinson, The Rand Corporation, "The Sino-Soviet Border Dispute: Background, Development, and the March 1969 Clashes"

John Gerard Ruggie, Dotation Carnegie Pour La Paix Internationale, "Collective Goods and International Organization"

David Sankoff, University of Montreal and Koula Mellos, University of Ottawa, "The Swing Ratio and Game Theory"

Kenneth A. Shepsle, Washington University, "The Strategy of Ambiguity: Uncertainty and Electoral Competition"

W. Phillips Shively, University of Minnesota, "Voting Stability and the Nature of Party Attachments in the Weimar Republic"

Lee Sigelman, Vanderbilt University, "Do Modern Bureaucracies Dominate Underdeveloped Polities? A Test of the Imbalance Thesis"

Philip D. Stewart, Robert L. Arnett, William Ebert, Raymond E. McPhail, Terrence L. Rich and Craig E. Schopmeyer, Ohio State University, "Political Mobility and the Soviet Political Process: A Partial Test of Two Models"

John L. Sullivan, Iowa State University and Robert E. O'Connor, Pennsylvania State University, "Electoral Choice and Popular Control of Public Policy: The Case of the 1966 House Elections"

John L. Sullivan, Iowa State University, "A Note on Redistributive Politics"

Herbert F. Weisberg, University of Michigan, "Scaling Models for Legislative Roll-Call Analysis"

Peter Willetts, Makerere University College, "Cluster-Bloc Analysis and Statistical Inference"

BOOK REVIEWS

Monarchy and Community: Political Ideas in the Later Conciliar Controversy, 1430–1450. By A. J. Black. (Cambridge: Cambridge University Press, 1970. Pp. 189. $10.50.)

In the 1950's Brian Tierney traced the canonist origins of conciliar thought, and Alan Gewirth analyzed and translated Marsilius of Padua. In the 1960's studies of the political thought of Gerson, D'Ailly, and Nicholas of Cusa by John Morrall, Francis Oakley, and this reviewer traced the further development of conciliar thinking. This volume moves the study of conciliarism one step further by looking at the thinking of the late conciliarists—principally but not only John of Segovia, and the anticonciliar papalists, mainly John Turrecremata, in the period from the split between Pope Eugene IV and the Council of Basel, to the papacy's final victory over its conciliar rivals. It also relates the efforts of the two sides to win the support of the European monarchs to "the birth of ideology . . . the peculiarly modern habit of justifying political acts by reference to abstract, metaphysical ideals" (p. 130) and argues that conciliarism "mediated between the medieval and modern conceptions of popular government" (p. 134).

In making the latter point, the author is at pains to distinguish the theories of the Council of Basel (1431–1449) from those of its better-known predecessor, the Council of Constance (1414–1418). Constance is seen as principally an emergency judicial response to the problem posed by three claimants to the papal throne, while the theorists of Basel are interpreted as asserting a general legislative supremacy of the representatives of the Christian church over their "minister," the Pope. Omitting most of the theological issues, Black sees the Basel conciliarists as carrying earlier corporation theory to its logical conclusion, when they asserted conciliar control of the papal financial and legislative system and finally in 1439 attempted to depose the Pope.

The argument is supported by extensive quotations from the sources and 32 pages of Latin excerpts from the writings of Segovia and Turrecremata. With its help we are able to fill in a further chapter in the development of constitutionalism in the West. The author's claims for the importance of the Basel theories in that development, however, raise some questions for this reviewer:

(1) Was the difference between the theories (as distinct from the practice) of Constance and Basel that great? Constance after all did assert in its decree, *Haec Sancta* (not, as Black and many other writers call it, *Sacrosancta*) that a "general council representing the Catholic church, has its power immediately from Christ and all men, including the Pope himself, are bound to obey it in matters pertaining to the faith, the extirpation of schism, and reform of the church in head and members." In addition, in the decree, *Frequens*, it called for regular convocation of general councils. (The Basel Council was called in response to the *Frequens* decree.) At least in the areas mentioned, Constance seemed to assert a general legislative supremacy for the council, and it has been so understood by a number of recent commentators in the Roman Catholic Church including Hans Küng and Francis Oakley.

(2) Black frequently refers to the democratic implications of Basel and its contributions to the development of popular sovereignty. Yet most of the participants and theorists at both Constance and Basel considered the councils as meetings of bishops and cardinals with an assigned role in the constitution of the church, which limited the Pope in certain areas but not in others. The main impulse of conciliarism was thus constitutionalist but not democratic. No one, with the exception of the discredited but influential fourteenth century theorist Marsilius of Padua (whom Black unaccountably persists in calling "Marsigilio"), called for representation of the laity. Only insofar as the two councils attempted to assert a general community control over the ruler could it be said that their theories implied some lay role.

It is true that the papalist opponents of the Basel rump-council accused it of revolutionary democratic tendencies. They were able to do so because it allowed priests and doctors of theology and canon law, (but apparently not laymen) to vote along with the few bishops who were left in the Council after 1437. That the papalists could attack this procedure with such telling effect, however, indicates how far the assumptions of the period were from anything that one could call democracy or popular sovereignty.

There is no denying however the influence of the *example* of Constance, and to a considerably lesser extent, of Basel, on constitutional controversies in sixteenth-century France and seventeenth-century England. The conciliarists' *theories* were less influential, although Francis

Oakley has traced a direct line of influence in "On the Road from Constance to 1688" (*The Journal of British of British Studies,* I [1962], 1–32). Even less influential were the theories of the papalists on the ideologies of the rising national monarchies. Black asserts a connection between theories of papal sovereignty and later similar monarchical theories, but his assertion is flawed by the admission that *all* papalist writers accepted the possibility outlined in canon law of papal deposition for heresy—and possibly for other reasons. More probable is the common need of both the papalist and monarchist writers to develop a rationalized, stable legislative and judicial system. The papal system came first, but *post hoc* does not necessarily mean *propter hoc.*

Yet despite the doubts expressed above about certain of the author's interpretations, I believe that this is an important book which tells a fascinating story in the development of Western constitutionalism. The story continues to be of interest as the constitutional struggles of the fifteenth century are now being repeated with increasing intensity within the Roman Catholic Church.

PAUL E. SIGMUND

Princeton University

A Dialogue Between a Philosopher and a Student of the Common Laws of England. By Thomas Hobbes. Edited with an Introduction by Joseph Cropsey. (Chicago and London: University of Chicago Press, 1971. Pp. 168. $7.50.)

Except for an edition published in Italy a decade ago, Professor Cropsey has presented us with the first modern separate edition of Hobbes's posthumously published *Dialogue.* He has endeavored to provide a more accurate text than is to be found either in Molesworth or in the original edition. His scholarly introduction runs to 48 pages (as compared with 115 pages for the *Dialogue*), and of this some 34 pages are devoted to a careful and exceedingly helpful analysis of the text. He also discusses such questions as when the *Dialogue* was composed, whether or not it is clear that Hobbes was the author, and whether it is right to assume, as has generally been done, that the work was incomplete.

The *Dialogue,* says Cropsey, "is to some extent a polemic against Coke" (p. 11). One must stress the qualifying phrase, "to some extent." Hobbes would appear to be concerned to reconcile his own teachings with those of the common law tradition—sometimes by explaining or qualifying, or even controverting, that tradition, but also sometimes by explaining the practical operation of his own theory in a way that makes it consonant with that tradition. It does not appear, however, that in this process he ever qualifies the main thrust of his own doctrine.

Yet it may well be that many readers of *Leviathan* will find much here to surprise them. Hobbes's defense of absolute sovereignty and of the absolute power of the monarch in a monarchical regime was never a defense of arbitrariness. His insistence on the right of the king, in the final analysis, to do whatever he judged necessary for the preservation of the realm and the safety of the people, even if that meant violating liberties or other constitutional provisions that he had granted, was never intended as an excuse for unreasonable conduct. Nor did he believe that that would be its consequence. In the *Dialogue,* written many years after the civil war, Hobbes perhaps felt less pressure to brush aside all elements of constitutionalism for fear of providing opportunities for trouble-makers and was more concerned to show the continuity of the prevailing regime, and of the type of regime he favored, with something approaching Lancastrian constitutionalism (although of course he did not call it that).

Never, in the *Dialogue,* does Hobbes use the phrase "the state of nature," although his substance gave him occasion to. Cropsey's explanation for this silence is persuasive. It is that Hobbes is shifting his emphasis. Without for a moment giving up the command theory of law (which he derived from his conception of the state of nature), he now wished to argue that the rationality of the laws was enhanced by the fact that, in practice, kings consulted representatives of the realm, in parliament assembled. Indeed, wrote Hobbes, ". . . there is no King in the World, being of ripe years and sound mind, that made any Law otherwise; for it concerns them in their own interest to make such Laws as the people can endure, and may keep them without impatience, and live in strength and courage to defend their King and Countrey, against their potent neighbours" (p. 166).

Other passages might shed new light on Hobbes, at least to the unwary reader of *Leviathan.* Repeatedly, for instance, he distinguishes sins from crimes. A sin is not a crime until it is made so by law. And yet the sins he refers to are not necessarily things that are, in the modern sense, "sinful." They may indeed even have been good in themselves before they were made crimes (pp. 78–79). Or they may have been evil; or, as we would say, "wrong." Although nothing here contradicts *Leviathan,* a different impression might have been gained by those who have read too hastily Hobbes's statement in his great classic that in the state of nature the notions of right and wrong have no place.

Another example of the same sort of thing may be found in the passage where the Philosopher asks how laws can be effective, even though they do embody the dictates of reason, "when the greatest part of Men are so unreasonable, and so partial to themselves . . ." (p. 58). Note that the charge is not against all men; but only against "the greatest part of Men."

About law as reason, he has much to say; nor can it be easily summarized, for he seems of a mind to take—at least to call attention to—the best of Coke on this subject, while insisting that it is kings (not lawyers and judges) who have made the law of England. But the law, he seems to concede, is the more rational because it has been fined and refined by a long succession of royal acts of legislation, with the advice of Lords and Commons.

For other matters discussed in the *Dialogue* and for a conspectus of its contents as a whole, the table of contents must suffice: Of the Law of Reason, Of Soveraign Power, Of Courts, Of Crimes Capital, Of Heresie, Of Praemunire, and Of Punishments. Hobbes students are indebted to Professor Cropsey for this scholarly and accessible edition of the *Dialogue.*

J. Roland Pennock

Swarthmore College

Latin American Political Thought and Ideology. By Miguel Jorrín and John D. Martz. (Chapel Hill: The University of North Carolina Press, 1970. Pp. 453. $12.50.)

The publication of this book just about doubles the broadly framed contemporary literature on this important element of the study of Latin America. Whatever the merits of the effort may be, however, it is still a long way from making a significant contribution to the discipline.

The book is divided into two parts. Part I describes the evolution of Latin American political thought during the nineteenth century. It is chronologically and conceptually organized around several themes and schools of thought. The several chapters focus on The Eighteenth Century Background; Natural Rights and Popular Sovereignty; Romantic Liberalism and Realism; Positivism; Idealism; and Anarchism. Part I was written by Prof. Jorrín before his death in 1965.

Part II, written by Prof. Martz, shifts the focus to a description and analysis of the major ideological movements of the twentieth century. It discusses Nationalistic Populism; Marxism; Social Democracy; Christian Democracy; International Perspectives; and Ideologies of Development.

Overall, *Latin American Political Thought and Ideology* is unquestionably a welcome addition to a scandalously infinitesimal literature. While more specialized studies of Latin American politics describe and analyze political parties, elections, voting patterns, labor unions, the Roman Church, the military and even peasant leagues, little has ever been offered by way of setting out the overall currents of thought and ideology. Some beginning has been made on the thought of Bolívar, Latin American positivism and the Mexican Revolution; exploratory forays have been launched to describe and analyze Latin American communism and, more recently, Castroism and the New Left; a little has been written on Argentine thought; and some bits and pieces probe contemporary ideological movements, but only two other scholars—W. Rex Crawford and Harold Eugene Davis—have attempted to put the whole business together in anything approaching comprehensive form. Even Crawford and Davis are not quite on target; both are concerned with more (or less) than political thought and neither is current enough to capture contemporary trends. In this sense, Jorrín and Martz have made a contribution. They have begun the job of bringing together some understanding of the meaning of Latin American speculation on political problems from 1800 to 1970.

But, however noble their intention, the authors fail to bring off the project as well as it deserves. The book is flawed. In the first place, much of the discussion of Part I wanders far afield in the realms of speculative philosophy and theory without offering a specified and clearcut analysis of *political* thought and *political* problems. Though Part II is more clearly in focus, the longish discussion of *indigenismo* in Chapter X again slips off into literature and social thought without quite explaining the movement's relevance to political ideology.

The lack of a chapter on Fascism, moreover, is incongruent in a study attempting to decipher important elements of modern and contemporary Latin American thought and ideology. In the 'thirties and 'forties Fascism exercised considerable influence in Latin America. *Peronismo,* perhaps second only to Castroism in its impact on contemporary Latin American politics, was strongly influenced by Fascism. Fascism also informed the ideology of Bolivia's

National Revolutionary Movement and Getulio Vargas' *Estado Novo*. A book that posits Anarchism as a significant strain of Latin American thought (and it is) misses an equally salient component of the milieu in its failure to deal with Fascism.

The absence of a discussion of the ideology and policies of the modernizing military is more understandable, though no less lamentable. The prototypical Peruvian regime is only three years old, and its significance was probably far from clear when the book went to press. Though some surely exaggerate the modernizing military as Latin America's wave of the future, it is bound to be a semi-permanent part of the continent's political panorama for the foreseeable future, and Latin Americanists are obligated to ferret out its philosophic and ideologic traditions and context. Furthermore, both Parts I and II (and perhaps the entire book) would have been strengthened by concluding chapters putting the material in context. Chapter gives way to chapter with only marginal attempts (*re* anarchism, for example) to integrate the various schools of thought, chronological evolution or ideological positions. The whole is always more than the sum of its parts, but this presentation of Latin American political thought and ideology makes little attempt to show how and why.

Beyond these substantial weaknesses, *Latin American Political Thought and Ideology* is marred by occasional organizational and stylistic infelicities. Particularly in Part I, the book gives far too much space to the presentation of biographic data. The study also falls off into historical chronology in several places—particularly in Chapter IX's discussion of Castroism. Elsewhere, an equally incongruent country-by-country framework is employed to describe and analyze an essentially transnational phenomenon—the description of *indigenismo* in Chapter X.

In sum, Jorrín and Martz have made a beginning, but the importance of the subject merits a more comprehensive and careful treatment.

EDWARD J. WILLIAMS

University of Arizona

Mathematical Sciences and Social Sciences. Edited by William Kruskal. (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1970. Pp. 83. $4.95.)

One of the series of reports by the Behavioral and Social Sciences Survey Committee, this book addresses the many problems arising from the close connections between the mathematical sciences and the social sciences. It is the joint product of the members of the Mathematical Sciences Panel, including (besides the editor) John Gilbert, Leo Katz, R. Duncan Luce, Alex Orden, and I. Richard Savage. Frederick Mosteller, though not formally a member of the panel, worked closely with it and wrote two of the chapters of the report—the initial chapter illustrating the whole focus of the study by an account of the use of mathematical models in psychological learning theory, and the chapter on "The Profession of Social Statistician."

Throughout the report the panel distinguishes three basic "mathematical sciences," which are sometimes considered much the same. These are mathematics proper—the study of formal symbolic structures; statistics—the study of making inferences from observations (most often on the basis of probability theory); and computation—comprising the variety of disciplines which have grown up around the modern high-speed computer. Each has its special value to the social scientist. Abstract mathematics is illustrated by Kenneth J. Arrow's axiomatic study of social choice, the use of calculus and linear algebra in economics, graph theory in social psychology, and group theory in the study of kinship groups in anthropology. Statistics has become so pervasive in all the social sciences that there are now recognized specialities in specific fields—econometrics, psychometrics, sociometrics, and so on. Modern computation has created great new flexibility in the use of mathematical and statistical procedures for analysis of the vast data banks long available to the social scientist but too unwieldy for earlier computational methods.

A major concern of the panel is how to meet the growing need for social scientists skilled in the analysis of the many problems which beset modern society. This demand is sharpened by the movement for a Council of Social Advisors and an annual Social Report, by the adoption of cost-benefit evaluation in program planning and budgeting, and by the frequency with which government is impelled to launch a program to meet an obvious problem only to find that basic data on which to build the program are lacking (as in the urban crisis and the war on poverty). The first formal recommendations presented by the panel are for the development and expansion of centers for training social statisticians, both in universities and in on-the-job training programs.

Of special interest to academic social science are the panel's recommendations in the field of computation. Along with the great advances in size, complexity, and speed of modern computers have come special difficulties. The report singles out several that plague social scientists in particular: the detection of obscure programming errors or undocumented program limitations, which may produce totally wrong results even though the results seem reasonable; the problem of numerical error structure, which social scientists (typically with modest mathematical training) are usually less well equipped to understand than are their physical science colleagues; and the conversion of most data banks from the standard Hollerith punchcard to a jungle of unstandardized magnetic tape systems—"tapes of different widths, tapes with the data recorded at different densities, and (probably most troublesome) tapes with the data recorded in different formats" (p. 69). The panel's recommendations to meet these and other difficulties deserve complete quotation:

> (1) Develop a computer sciences curriculum for social science students that is not based upon impractically exigent mathematical prerequisites.
> (2) Encourage more individuals to become proficient in both computing and behavioral science by establishing suitable faculty positions and appropriate paths for professional development.
> (3) Stress the importance of program documentation, both for local usage and as a requirement for scholarly publications.
> (4) Support measures to put the educational usage of computers within the reach of educational budgets (pp. 72–3).

The thorny problem of how to teach the mathematical sciences to social science students is fundamental, and the panel appropriately reserves it for its final chapter and its final recommendations. Who should teach the mathematical sciences? One proposed solution (not the only one possible since much depends on the specific individuals in the departments concerned) is to have the basic courses taught in the mathematical departments and some specialized courses taught in the social science departments. What mathematical topics? The panel agrees with Herbert Simon that the calculus is the basic entrée into most areas of the mathematical sciences. Social science departments should actively use in their own courses and seminars the competence assumed of their students by their mathematical science prerequisites—meaning that "a substantial portion of the social science faculty should have similar competence" (p. 80). Theory versus methods? Some mathematical courses fail to interest social science students because of so great an emphasis on abstract theory that "all sense of application is lost"; social scientists, on the other hand, are often impatient with theory and want to know simply "how to do it" (p. 81).

The panel recommends at least some theoretical work; the inclusion in mathematical courses of motivating examples drawn from the social sciences; use of machine programmed teaching material for social science students in algebra, calculus, numerical analysis, matrices, statistics, and computer programming; development by the mathematics departments of courses including advanced topics of special interest to social scientists (Markov chains, matrix theory, numerical analysis, graph theory) without the rigor of advanced mathematical abstraction and numerous prerequisites; the use of joint faculty appointments in mathematical and social science departments—perhaps extending even to a distinct social mathematics or social statistics department; and, because of the need for training students in real world problems with real world data, the establishment of more schools of applied social research.

The detailed recommendations in the reports of the Behavioral and Social Sciences Survey are the responsibility of the panels preparing them and are not necessarily approved by the parent organizations. Nevertheless, this volume presents the most authoritative statement we are likely to get of the problems involved in quantitative social science, as well as the most authoritative set of recommendations on what to do about them. The National Academy of Sciences and the Social Science Research Council, cosponsors of this and the other reports of the Survey, are to be commended for this product, which will be helpful to all political and social scientists interested in using mathematics, statistics, and computer science more intelligently and in attacking the social problems with which their disciplines are concerned.

OLIVER BENSON

University of Oklahoma

The Growing Church Lobby in Washington. By James L. Adams. (Grand Rapids, Michigan: William B. Eerdmans Publishing Co. 1970. Pp. 294. $6.95.)

This timely book, with a foreword by Charles P. Taft, views the rapidly increasing economic lobbying by the leading church groups in the nation's capital through the untinted glasses of a newspaper reporter. Mr. Adams, from the *Cincinnati Post & Times-Star,* has long been a specialist on religious news and writes with an easy acquaintance with the subject.

The time was, not long ago, when church lobbying was neither admitted nor written about. One of the pioneering books (L. E. Ebersole's *Church Lobbying in the Nation's Capital,* 1951), although startling at the time, now seems rather innocent because the church lobbyists have shifted their emphasis from "moral" issues to hard-core economic self-interest. Early church political activity took the form of "petitioning" the various legislatures to mitigate or abolish slavery, then passed on to Comstock-type laws involving moral conduct, and then to agitation for prohibition. Here we learn that the greatest pressure is for acquiring federal and state public funds to help operate church schools and hospitals, church-administered aid to the needy at home and abroad, and even vast building programs for church-operated or church-related colleges dedicated to the propagation of their particular beliefs. In Washington alone, Adams estimates that the church lobby spends a million dollars annually.

Adams of necessity concentrates on the Washington church lobby and then, more specifically, focuses attention on the drive for federal aid for church schools. He provides a detailed step-by-step account of how the coalition among Protestants, Roman Catholics, and Jews first began to function on the same side of the fence with organized labor in the 1963 drive for the Johnson Civil Rights Program. The moral thrust and the political know-how gained by this new alignment of forces carried over immediately and with great effectiveness to the drive for the Johnson Education Bills of 1965. This new thrust was greatly fortified, Adams points out, by the emerging ecumenical spirit paralleling the new compassion for the disadvantaged blacks who presumably were to be happily rehabilitated through education. The resulting climate of opinion put opponents of the education bills in the position of virtually opposing God and country. With all this going for the bills, enactment was assured.

But the author wisely warns us that it is easy to be deluded, as were some of the church lobbyists, into thinking that they alone put the bills through. Actually, Adams substantiates the conclusion that Johnson's "power" was sufficient at the time to have them enacted on his own; doubtless the substantial congressional margins would have been less impressive without the lobbyists' efforts. In fact, the Johnson drive, we are told, was so great that it caught by surprise the Protestant and Jewish leaders who did not have access to the inside information enjoyed by their Roman Catholic colleagues. Adams devotes several later chapters to some of the aftereffects and afterthoughts about the church lobbyists, the laity, and the public. He senses a growing public cynicism regarding the special tax benefits and other privileges enjoyed by churches in the American economic and social system.

This developing negativism was later substantiated by Leo Pfeffer in the Winter, 1971, issue of *A Journal of Church and State.* Some of the leaders of the Negro community, for example, are becoming hostile toward the use of public funds to support a side-door escape from integration in the church schools. Pfeffer presents some convincing statistics to indicate that this is, indeed, what may be happening in the church schools.

This book should interest several groups of readers: Students of pressure-group politics, the growing numbers of clergymen and laymen concerned with church-state relations, and the newspaper fraternity itself which tends to handle the subject either superficially or with extreme caution. The writer has drawn heavily upon primary sources, but he rarely provides the reader with specific references—doubtless they may be found in his own notes. It is disappointing that there is no bibliography.

R. WALLACE BREWSTER
The Pennsylvania State University

Law and Justice: Essays in Honor of Robert S. Rankin. Edited by Carl Beck. (Durham, North Carolina: Duke University Press, 1970. Pp. 358. $12.00.)

The unifying theme of this collection of essays honoring Professor Robert S. Rankin is an emphasis upon law and justice. The variations upon that theme are as diverse and generally as intellectually stimulating as a musical counterpart by Johannes Brahms. As indicated in the introductory essay the emphasis upon law and justice was designed to provide contemporary counterpoint to the national emphasis upon law and order.

Taylor Cole and John H. Hallowell have contributed a gracious biographical sketch in which they provided impressive evidence to the profession at large that the academic and public career of Robert Rankin successfully combined insistance upon empiricism and a deep personal commitment to the fulfillment of relevant political and social objectives. This portrait is complemented by Floyd M. Riddick's essay entitled "State and Local Government: Scholar and Participant;" this details Robert Rankin's public service career in state and local government, which was paralleled by his academic labors until his appointment as a member of the U.S. Commission on Civil Rights.

The substantive articles which comprise the major portion of this collection cover a very wide range of subjects. A number of the essays may appropriately be classified as contributions within the broad framework of what Herman Pritchett referred to as traditional constitutional law. These consist of several excellent articles analyzing particular constitutional doctrines or assessing a clearly defined subject matter area with regard to the development of legislative policy and judicial interpretation, e.g., S. Sidney Ulmer, "Searches, Seizures and Military Justice;" Claud H. Richards, Jr., "Religion and the Draft: Jehovah's Witnesses Revisited"; Francis Canavan, S. J., "Constitutional Casuistry: Cases of Conscience"; Spencer R. Gervin, ". . . And Kids Have Rights Too"; Joseph L. Bernd, "Equal Protection of Voting Rights: The Logic of One Person, One Vote"; and John A. Morgan, Jr., "From *Maxwell* to *Duncan*—Progress or Regression?" These essays range from constitutional case analysis to treatments which focus on the impact of Supreme Court decisions upon Congress (e.g., "From *Maxwell* to *Duncan*—Progress or Regression). Clarence N. Stone devotes appropriate attention to Congressional reaction to specific criminal procedures in his article entitled: "Patterns of Voting on Mallory, Durham and Other Criminal Procedures in Congress."

In related essays several overall analyses of the current status of basic human rights are provided, as, for example, in Philip B. Secor's "Academic Freedom in Political Context: The North Carolina Speaker-Ban Law." Robert C. Clute, Enid Campbell, and W. D. K. Kernaghan summarize civil rights developments in selected foreign countries. Alfred O. Canon analyzes electoral developments in Memphis, Tennessee, in "A Negro Candidate for Mayor in the Urban South." Charles B. Hagan explores "Cigarettes and Public Policy: The Inauguration of a New Policy" and, finally, Laurence E. Noble, Jr. wrote a challenging essay entitled "Reflections on Political Change."

The essays are of uniformly good literary quality. The multiplicity and diversity of these essays do not permit broad or sweeping commentary. Suffice it to say that several of them are excellent expositions. Thus Sidney Ulmer's analysis of "Searches and Seizures and Military Justice" is characterized not only by precision and commendable parsimony in its descriptive portion, but by incisive comparative emphasis upon the similarities and differences of civilian and military justice. Similarly, Clarence Stone's "Patterns of Voting on Mallory, Durham and Other Criminal Procedures in Congress" embodies appropriate utilization of roll-call analytical methods as well as conventional commentary.

In a rather remarkably relaxed fashion, the numerous contributors have captured successfully the sense of intellectual diversity and felicitous spirit of the fine scholar and public servant for whom this collection was written.

JOHN R. SCHMIDHAUSER
University of Iowa

American County Government; With an Annotated Bibliography. By John C. Bollens in Association with John R. Bayes and Kathryn L. Utter. (Beverly Hills, Calif.: Sage Publications, 1969. Pp. 433. $17.50.)

The annotated bibliography takes up all but 87 pages of this book. It is the major contribution of the author and his associates and will be an excellent reference since hardly any significant work on county government is omitted. The annotations unfortunately are not critical, simply descriptive. The review of state and local textbooks indicates which ones devote attention to counties.

From a review of the annotations, it is quite clear that there is little sophisticated work on county government. Few political scientists give it more than passing attention. Certainly one would have to agree with Bollens that the literature "is structural, descriptive, and legalistic. It is prescriptive and pragmatic. Conversely, it is not systematic, behavioral, or theoretical. . . . In sum, the literature is incomplete" (pp. 12–13).

In the opening sections, Bollens suggests where we need to go in our studies of county governments, as others have suggested in municipal and state governments. In fact, he feels that some of the newer approaches to the other levels might be utilized by students of county government. He develops an "Index of Political Vitality," which he describes in some detail. But unfortunately, he does not attempt to tell us how to use the index and more importantly what it will tell us, except to suggest that without political vitality "either existing or potential, a local unit is a meaningless relic and a poor investment" (p. 40).

One problem with *American County Government* (a project supported by the U.S. Department of Commerce under the Public Works and Economic Development Act of 1965) is that it deals with 3,049 units of local government called counties. Thus, as the author would agree, we talk in some cases about sparsely populated rural units that perform few functions and in others about major modern, urbanized units like Los Angeles County. It is interesting to know that Petroleum County, Montana (1950 population, 1,026), has a county manager, but not very important unless you come from Petroleum County. On the other hand, many people will want to know how Cook County, Illinois, is handling welfare and environmental control.

County governments are worth studying, particularly in urban areas. The role counties play in some urban areas may be enlarged now that the one-person, one-vote ruling is filtering down to the county level. This could well mean that counties in some parts of the country will now be concerned mainly with urban problems. Many feel that because of their size, counties will be the governments of the future in some urban areas, as suggested by recent restructuring in Jacksonville, in Indianapolis, and elsewhere. The evidence on this trend, however, is not in yet.

In some areas of the country, such as New England, counties are dying; in others, such as California, they are vital, active units taking on new functions and modernizing old ones. We need to know more about counties if we are to understand them and their role in a broader political perspective. But any attempt to measure them across the country will not help much. The author's suggested index of political vitality with five components of (1) resource utilization, (2) volume of intergovernmental linkage, (3) changes in organization and processes, (4) adequacy of public accountability, and (5) extent of voting and competition in elections, will tell us little except that some counties are performing needed services, others are not, some are tackling current problems with the latest methods, others face problems little changed for fifty years; or some counties are in the mainstream of the region's political system while others are not.

At this time and place in the development of the country, we should probably concentrate our studies on urban areas and their governments. As we probe, we may find that county governments are indispensable in the delivery of services to the people who need those services. If counties are necessary for the delivery of services they will be worthy of our study. If not, we might as well ignore them. When enough people have made enough similar studies, we might be ready to come out with a revised volume called—to paraphrase an earlier work—"Big City and County Politics."

SAMUEL K. GOVE

University of Illinois

Democrats of Oregon: The Pattern of Minority Politics 1900–1956. By Robert E. Burton. (University of Oregon Press, 1970. Pp. 158. $7.50.)

The general election in 1956 was a critical one in Oregon, a high point in the rebirth of the Democratic Party which had been the minority party in the state for more than half a century. The Democratic candidate was elected Governor, Wayne Morse won another term in the Senate for the first time as a Democrat, three Democratic Congressmen were elected, and the lower house of the State Legislature came under Democratic control. In this book, historian Burton tells the eventful story of this renaissance of the Democratic Party.

Burton begins his account at the turn of the century when the Democratic Party was weakened by division between Silver and Gold Democrats. During the Harding to Hoover era, a motley group of Conservatives and party hacks led an ineffectual and disorganized minority party, so that when Franklin D. Roosevelt came to power in 1932 many party leaders opposed New Deal policies. Younger Democrats who were New Deal supporters joined a new organization, the Commonwealth Federation, which sheltered homeless liberals and became the organizational nucleus for new leadership that would reconstitute the party. World War II and its aftermath was a period of population growth and economic expansion which brought an influx of new Democratic voters. Several other factors contributed to the growth of the Democratic Party. Politically conscious CIO unions were formed during the thirties and forties among lumber workers. Public power became an issue that forged an identity of political interests between farm and labor organizations. New political leaders, notably Monroe Sweetland and Howard Morgan, fought the entrenched old guard of the party, and aided by President Harry Truman, reached positions of party leadership in 1948 and 1950. From then until 1954, the Democrats produced a new generation of able candidates: Senator Richard Neuberger, Edith Green, Charles Porter, Al Ullman, Robert Holmes, and others.

The patterns of change described in this Oregon chronicle are not unique. They resemble the rebirth during the postwar years of the

Democratic Party in Michigan, Minnesota, and Wisconsin. The contributing factors are parallel: the growth and relocation of industrialization and urbanization, the crystallization of new issues, the rise of a new political generation whom Roosevelt inspired, Truman nurtured, and Adlai Stevenson ironically fulfilled. But the Oregon development has some uniqueness. The change in party fortunes has not altered the style of Oregon politics, which the author characterizes as "nonpartisanship, independence of party organization, and the politics of personality." To this might be added a subdued style of politics, rampant and capricious factionalism, and a deepseated populism.

Burton has written a lucid and coherent account of party politics which emphasizes elections, issues, personalities, and factional maneuvers. But there is more narrative than analysis. The clash of interests and the dynamics of electoral behavior are given inadequate treatment. Only one paragraph tells us that

> Oregon's measured growth, the religious, ethnic, and racial homogeneity of its small population, and an economy dominated by the resources within the boundary of the state have combined to produce a high degree of intrastate cohesiveness. This, in turn, prompted a relatively stable attitude toward politics, a contrast to some states where cultural diversity and a rapid industrial pace nurtured continuing conflict (p. 5).

On the whole, this is true, but if one looks closely at the politics of some communities in Oregon, partisanship is intense, and cleavages are sharp. The absence of highly organized political parties doesn't necessarily indicate political consensus. We have become sophisticated about party competition and strong and weak party organizations. The analytic usefulness of such terms is limited when applied to states and communities where party organization is diffuse. When party apparatus is only a paper organization, party leaders do not act in concert, and middle level and party workers are in disarray, what then does party mean? The paradoxical answer is that often party attachments are woven into the fabric of cliques, social circles, religious denominations and occupational associations. A party organization, developed or not, may conceal durable political primary groups that make party organization unnecessary.

The author neglects other factors which deserve extensive analysis. The social differentiations in Oregon society reflected in voting data could have pointed to the continuities and change in electoral behavior. Regional differences and economic interests, expressed as they were in the politics of the state legislature, were significant in the rise of the Democratic Party. The part that interest groups and party factions played in sponsoring candidates deserves greater attention. The author stresses the politics of personality, but does not probe it. When the issues divide and when elections are without issues, what explains the tendency to cluster around personalities? Is it perhaps just that politics and everything else is "personal" in small-scale political activity?

Overall, Burton's history narrates well the events in changing party politics. But that isn't enough. More and more historians are joining other social scientists in making explicit the hypotheses that guide their explanations of historical change. When they do so, they find rich use for sociological, economic, and political data. When historians ignore the perspectives of other social sciences they usually invent their own psychology, sociology, or economics. A broader perspective and further analysis would have enriched this fine study.

LESTER G. SELIGMAN

University of Oregon

The Higher Circles: The Governing Class in America. By G. William Domhoff. (New York: Random House, 1970. Pp. ix, 367. $7.95.)

Professor Domhoff undertakes to make the case for the proposition that America has an "upper class," that it is a social class, cohesive and self-conscious, and that although it does not "control" policy (pp. 105–06), it does "dominate" it. The last section of the book takes on two sets of critics of his position—the Right (which his own view resembles) and the Pluralists, for all of whom the late Arnold Rose is made surrogate. Maurice Ravel said of one of his popular pieces—in disparagement—that it was "orchestral tissue without music." Professor Domhoff does all right with the music—the tune is a very familiar one—but supports it with a flimsy tissue of pop sociology. The existence of an upper class seems to be an article of faith with the author, an *a priori* construct, and one is reminded of Cabell's *Jergen* where Heaven and the God of Jergen's grandmother are created because she believed they existed.

Professor Domhoff creates the upper class out of the *Social Register* (coverage—thirteen American cities), the opinions of newspaper society editors, lists of directors, trustees, law partners and other such grandees in 15 American cities, *Who's Who of American Women,* alumni and alumnæ bulletins of private schools, biographical and autobiographical materials, and novels about "high society." Although C. Wright Mills treated celebrities as something apart from his power elite, Professor Domhoff says that Mills was mistaken. The jet set is the upper class at play (p. 70).

One of the wonders of this construction is that the upper class is the *only* class in America, which is like having one bookend. Professor Domhoff is willing to say that something like classlessness exists at all levels of society until you get to the top (p. 74). There the "continuum hardens into a social class with more or less definite boundaries and class consciousness," and cohesiveness. The evidence for cohesiveness is that everybody "is related to everyone else, and sometimes many times over" (p. 76); rich kids go to "a handful of exclusive institutions"; upper-class clubs have nationwide memberships; the upper class lave their upper-class bodies at the same summer resorts, exercise them in the snows of the same winter resorts, and adopt different life styles from "the rest of the population" (p. 88). In one confusing sentence it is also said, "Finally, after all this, there is death, but it is not the great leveler it is claimed to be when it comes to differences in life style" (p. 90). At funerals, upper-class people cry less, practice cremation more, restrict viewing of the body, and bury it faster than just folks.

The evidence for the second proposition—that the upper class is class conscious—is anecdotal, impressionistic, journalistic, and inferential. Professor Domhoff says that for lack of hard data it is necessary to rely upon the testimony of the occasional insider-expert like E. Digby Baltzell, on novels like those of J. P. Marquand ("a perceptive observer"), and "remarks about the 'nobility' and 'peasants' that used to slip from cultured tongues. Then too, every once in a while an upper-class writer will drop a remark that implies class consciousness" (p. 93). He did, however, circulate a questionnaire to one-third of the 912 Freudian analysts in the country and all of the 70 Jungian analysts to find out what they had to say about the class consciousness of their upper-class patients. He got only 36 replies, but 27 of the 36 thought that upwards of 70 per cent of their upper-class patients were class conscious. It is not said whether this is what brought them into analysis.

The trouble with this kind of demonstration is that it is largely suppositious and subjective. Class is not defined in terms of social and economic functions but in terms of personal attributes. Now, everybody knows that there are rich men and women, that rich folks have pretty ways, that some play hard and work themselves into cardiac and ulcerous bad health, that others do nothing except glow with gold or shine like dead mackerel and when all this is said, what one has said is that there exists a status group with the indicated qualities. Worth consideration is the view that our society is characterized by "nonegalitarian classlessness," as Stanislaw Ossowski has said in his *Class Structure in the Social Consiousness*—a form of social inequality without social stratification, with much overlapping, without clearly differentiated social and economic functions, unlike the concept of class with which Marx worked. All "classes" in effect do the same thing—they tend the bureaucratic machine—and if there are indeed social differences, they tend to blur, making integration as much a descriptive model as conflict to characterize social and economic relationships, as Marcuse has argued in *One Dimensional Man.*

Having established the existence of a cohesive and class-conscious upper class, Professor Domhoff is then required to show how it operates as the "governing class in America." In his previous work, *Who Rules America?* Professor Domhoff purported to show how the upper class controlled the executive branch of the federal government. His current claim is more modest. Now they only control "most major corporations, foundations, and private universities," but they do not control the political process. The influence that they have in the political process is "dominance," which means that "members of the upper class sit in pivotal governmental offices, define most major policy issues, shape the policy proposals on issues raised outside their circles, and mold the rules of government" (pp. 105–06), although it is also said that "relatively few members of the upper class concern themselves with political matters." The demonstration of dominance is undertaken in the areas of foreign policy, social legislation, and the "struggle for minds." Although the Council on Foreign Relations is said to be the key middle term between the corporations and the federal government in foreign policy, the author says that he cannot demonstrate the influence of the CFR on any single act of policy. Its study groups, however, lead to the publication of important works like Henry Kissinger's *Nuclear Weapons and Foreign Policy,* and of course Henry Kissinger winds up in the White House, and we may indeed have a demonstration of the upper class at work, although Henry Kissinger is not an Anglo-Saxon Protestant, never went to Groton, and may or may not eventually be cremated.

A close parallel to Professor Domhoff's discussion of the Council on Foreign Relations is the treatment of the Institute of Pacific Relations some two decades ago by the Internal Security Subcommittee of the Senate Judiciary Committee. Like Professor Domhoff and the CFR, the McCarran committee said that IPR activities were made possible largely through the support of American corporations, industrialists, and foundations; that it set up active and cooperative relationships with government officials in the foreign policy field; and that it achieved "close organic relationships with the State Department through the interchange of personnel, attendance of State Department officials at IPR conferences, constant exchange of information, and social contacts." Like the McCarran committee also, Professor Domhoff is suspicious of the academy, and in a piece of insinuation worthy of his predecessors in the early 1950's he says, "While I do not believe for a minute that the power elite dictate to these scholars [who participate in CFR activities] as to what they should say, it should be clear that members of the power elite see no reason to discontinue their support of such efforts" (127).

Professor Domhoff says that there are differences between him and the ultra-conservative Right. He does agree with them that the country is run by rich, Eastern, Ivy League-educated, well-traveled, businessman, internationalist, *Social Register* types many of whom "travel under the label of liberals"; and he joins them in thinking that they have some justification for being angry about the welfare state. But he does not accept the ultra-conservative concept of the power structure embodied in the phrase the "international Communist-Jewish conspiracy" because the internationalism of which he speaks is not ideological but economic; there is only a handful of Jews in the upper class and they "are more upper class than Jewish" (p. 297); and "Whatever we may say of him, David Rockefeller is not a communist" (p. 291).

In his last pages, Professor Domhoff disclaims conspiratorial theories of upper-class control or dominance, but he chides pluralists for underestimating the degree to which there is unity, consciousness, and manipulation in the social group that is the subject of his book, and he says, "If pluralists ask just how unified, how conscious, and how manipulative, I reply that they have asked a tough empirical question to which they have contributed virtually no data —at the same time pointing to findings such as I have presented throughout this book" (p. 299). So, it would seem to be up to the pluralists to prove Professor Domhoff's case for him, since the findings of the book do not support the conclusion with which he started.

EARL LATHAM

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The Politics of Rescue: The Roosevelt Administration and the Holocaust, 1938–1945. By Henry L. Feingold. (New Brunswick: Rutgers University Press, 1970. Pp. xiii, 394. $12.50.)

The hideous reality of the destruction of European Jewry by the Nazi Germans has been so difficult for us to accept as having actually occurred that we have avoided contemplating this event, analyzing its course of development, or relating it to our theories of political behavior, which seem so feeble in the face of such barbarity. Instead with notable exceptions such as Raul Hilberg, we have debased the meaning and implications of this catastrophe for modern Western civilization, or we have attempted to excuse the central actors by spreading the blame in ways that remove the focus from the fact that the Nazis really did commit genocide upon the Jews of Europe. Thus, we have trivialized the word "genocide," using it to describe a range of unpleasant activities; almost any policy or program which is disapproved these days becomes "genocide." Massive multi-billion-dollar programs to improve health, redistribute income, provide for welfare, redevelop urban centers, etc. are called genocide, as in this statement by a group of New York City Urban Fellows in a 1971 American Assembly document: ". . . We recognize that everyday government is a party to genocide by its inaction and the active intent of some of its agents." Everyday government! In America!

But to equate any and all grievances with "genocide" is as ridiculous as to attempt to find some other guilty parties for the Nazis' crimes. Some years back Pope Pius XII and the Roman Catholic Church seemed to be leading candidates in this share-the-guilt process. Then Arthur Morse in his polemical *While Six Million Died* tried to extend the guilt to the American government under the leadership of Franklin D. Roosevelt. Even worse was the implication of Hannah Arendt's study of the Eichmann trial that it was the Jews themselves who were at fault for not having resisted the Nazis more vigorously. If all are guilty, even the victims, then no one is really guilty, and what can we say or learn about the specific fact that the Nazis, and only the Nazis, organized and directed (with collaborators of course) a complex system of literal mass murder that succeeded in its purpose of making Europe "clean of Jews"?

We need scrupulous scholarship as a corrective to the looseness of thought and language, the evasiveness, and the myths of the dispersion of guilt before we can begin to estimate what political science theory can contribute to an understanding of the holocaust, and Henry L. Feingold has provided us with such a corrective to Morse's sincere, but overstated case against the Roosevelt administration.

The facts, however, are ugly. While the Nazis were announcing their intentions publicly, and more secretly planning and implementing their "final solution" of the Jewish problem, i.e., extermination, the American government was aware of the great jeopardy of the European Jews. Yet, despite warnings, pleadings, moral appeals, and political threats, the United States did virtually nothing to succor the masses of Jews being killed by the Nazis.

How could this apparent passivity have occurred under F.D.R., whose administration and advisory circle were studded with Jewish luminaries who created what Feingold calls a "litany" of names—Morgenthau, Frankfurter, Rosenman, *et al.*? In Congress there were leaders like Bloom, Sabath, Dickstein, and Celler, and there was a host of private agencies devoted to the refugee problem. How could this government have remained indifferent to its Jewish and humanitarian impulses to provide relief and safe haven for the threatened millions?

As Feingold points out, the explanation for the American failure to act is compounded of diplomatic, legal, and high war-policy reasons combined with domestic political considerations, as well as less elevated phobias against a stream of refugees including possibly some spies or saboteurs. The Roosevelt administration did take the initiative in calling the Evian Conference in 1938, which led to the establishment of the ineffectual Intergovernmental Committee for Political Refugees, and, again, during the war, the Bermuda Conference in April 1943 met to review the plight of the Jews and others suffering under the Nazis. The scope and seriousness of the problem were not fully realized until much too late, when the War Refugee Board was created in 1944; by the time it was in full operation the war was almost over, and the European Jews were almost all dead.

Breckinridge Long, Assistant Secretary of State, who was primarily responsible for the administration of immigration law and refugee policy during the war, clearly resisted any liberalization of policy, and Long was supported at key junctures by F.D.R. personally. American policy was simply to win the war as quickly and decisively as possible, and this policy was believed to be the best answer to a host of problems, including what was considered the refugee problem. Atrocity stories had been punctured after World War I, and there was a deep skepticism concerning reports about Nazi horrors. Any move to liberalize the restrictive immigration laws, it was feared, would call forth a right-wing reaction that could harm the war effort. Our allies were equally coy about what to do about locating large numbers of refugees, and little despots like Trujillo in the Dominican Republic were not above playing games about admitting immigrants to embarrass their democratic neighbors. But above all, American policy makers simply did not believe that the Jews were being exterminated, despite the intelligence reports, and their reactions continued to take conventional forms—dispatch of relief funds, food and medical supplies, censuring the persecution of minorities, providing for refugees, and adjusting or enforcing immigration quotas. That the Germans were not merely persecuting the Jews, but were exterminating them did not finally sink into the consciousness of American policy makers until the death camps were opened by the liberating armies.

Professor Feingold has had the unusual courage to look at this maelstrom of events, actors, programs, and policies in the finest tradition of committed historical and analytic scholarship. His book is based on the thorough review of the primary sources one would expect in a work originating as a doctoral dissertation. As he shows it to us, American policy making in the face of Nazi crimes was so stupid and repulsive as to make one despair at our insensitivity and incompetence, even given the understandable limitations of imagination and will that foreclosed more direct and vigorous action to save a larger remnant than that which did survive the Nazi murder apparatus. Those interested in demythologizing the Roosevelt administration's role in relation to the holocaust can rely on Feingold's excellent volume for the details of policy, diplomatic exchanges, and accounts of conflicts among those interested in rescue and relief, and between them and the cautious and dilatory State Department bureaucrats.

For political scientists interested in theory, this book will not provide much encouragement. Our debates about pluralism and elitism pale into explanatory insignificance when one contemplates the enormities considered in Professor Feingold's book. Reading it is a sober and necessary experience for those whose ambitions still lie in elaborating general theories of political behavior.

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Agenda for a City: Issues Confronting New York. Edited by Lyle C. Fitch and Annmarie Hauck Walsh. (Beverly Hills, California: Sage Publications, 1970. Pp. 718. $17.50, cloth. $4.95, paper.)

Having inflated the expectations of America's universities and underdeveloped nations elsewhere, the Ford Foundation has now decided to assist our central cities. Hence, among its various benefactions, a grant to New York's Institute of Public Administration to "analyze options open to the city administration taking office in 1970." And thus this advance agenda, its assignments commissioned prior to polling day, intended for whoever was going to win the 1969 elections. The purported result is a series of statements by accredited and nonpartisan experts, equally available for a John Lindsay, a John Marchi, or a Mario Procaccino. The volume provides a valuable case study—of how Ford and one of its prominent handmaidens undertake to counsel a city.

The book consists of 16 principal papers, five of them accompanied by critical "commentaries." Obviously the Institute of Public Administration wanted men of sober stature, sound judgment, and clearheaded experience, who could give the next mayor advice on how to cope with the tumultuous 1970's. It would have been fascinating to have sat in on the discussions where possible contributors were considered: a transcript of those sessions would illuminate much about the thinking which goes on in our more insulated institutions. At all events, the upshot was to turn to people who had ringside seats during the early 1960's. The articles on health, education, and welfare, for example, were assigned to men who had served as Commissioner of Health, Superintendent of Schools, and Commissioner of Welfare under Mayor Robert Wagner. In other words, former stablemates of Lyle Fitch, who happens to be editor of this volume, is head of the Institute of Public Administration, and was City Administrator during Wagner's tenure. They tell us how well they ran things and how much was accomplished while they were in charge.

And for "race relations," whom to invite but Nathan Glazer, who—in company with Daniel Patrick Moynihan—explored the city's ethnicity in this same era? His chapter, really a rearranged version of a 1970 preface to *Beyond the Melting Pot,* tells the city that it should give greater encouragement to its Negroes, the better to undercut the appeal of its Blacks. The analysis of the Mayor's office comes from Wallace Sayre, who celebrated the city's pluralism as it was arranged back in 1960. He writes about that office almost as if someone named John Lindsay had never been mayor. And so on with a predictable roster: Roger Starr on housing, Dick Netzer on finance, and Jameson Doig on crime and the police. (Was James Q. Wilson tied up that week?) Some interesting information emerges: in 1968, for example, the 144,351 households in the city having incomes over $15,000 on the average paid less than 12 per cent of their earnings in rent. But no surprises.

Symposiums of this sort always enlist one house maverick, brought in to be "provocative"—as chairmen are prone to put it in their introductory remarks. In this instance, Adam Walinsky joined in submitting a plan for decentralizing the city: not particularly original, considering how many blueprints of this sort are around, but refreshing nonetheless. However lest readers be carried away by Walinsky's impracticality, the editor recruited not one but two commentators in this case—Herbert Kaufman and Harvey Mansfield—who reviewed Walinsky's paper and graded it C+. (Its proposals, Kaufman concluded, "do not inspire confidence.") Throughout most of these 718 pages we see the city from the administrator's window, antedated to the days when bureau chiefs encountered much less caviling from opinionated citizens.

Given its auspices, we might expect an emphasis on official authority and professional prerogatives. Yet while such an approach may have been viable in earlier decades, it fails to come to grips with the kind of people who are now the subjects and objects of urban administration. There is an air of wistful thinking on these pages: if only eight million New Yorkers would consent to act as constants in the administrative equation, if they would only keep quiet and hold still, they could be efficiently serviced. However *homo urbanus,* at least in New York, has become a much more obstreperous creature in recent years; and governing him calls for talents nowhere elucidated in this volume. Strikes, sit-ins, and subtle and not-so-subtle sabotage are now the norm, with high school principals, police commanders, and welfare mothers continually available for confrontation. Most of the contributors all but omit mentioning a man named John Lindsay, and hardly any suggest that his mode of operation may have something to be said for it. As an administrator he may not have adhered to the best of protocols, but in possessing a personality and impressing it on the city, he has singlehandedly raised the spirits of all New Yorkers—including those who hate him—at a time when urban problems outrun the resources of available agencies. There is almost a grudging quality in this omission: the reaction of writers mistrustful of anyone who unsettles conceptions of governance a decade out-of-date.

If New York needs an agenda, I would much rather recommend the columns of Jimmy Breslin, Murray Kempton, and Pete Hamill, along with the observations of Bella Abzug, Charles Rangel, and any random group of parents in Bensonhurst or Bedford-Stuyvesant. Indeed, had the Ford Foundation solicited the views of the janitors and cleaning women who service its air-conditioned East Side offices, it would have come up with a much more useful and certainly a far less costly compendium.

ANDREW HACKER

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Public Expenditures and Policy Analysis. Edited by Robert H. Haveman and Julius Margolis. (Chicago: Markham Publishing Company, 1970. Pp. 596. $11.95, cloth; $6.50, paper.)

Analysis and evaluation of governmental expenditure programs have long been a challenge to both the practitioner and theorist of public administration. Particular concern has been devoted to achieving more rational analysis in the choice of policies and allocation of resources to a growing number of public programs. From the beginning of the budgeting movement in government to the present, the quest has yielded major milestones, ranging from A. E. Buck's treatise, *Public Budgeting*, through monumental efforts in methodological improvement by the Bureau of the Budget in the 1940s and 1950s and the introduction of performance budgeting, to the most recent attention by economists in the concept of "PPBS"—the Planning, Programming, Budgeting System. Professors Haveman and Margolis have made a significant contribution in this collection of twenty-six papers which is intended for use in "Economics and Political Science Departments and Schools of Business and Public Administration."

The thrust of the papers is almost wholly economic and illustrates high quality scholarship of noted authorities in the field of public finance. Conceptually its range is wide, encompassing discussions of microeconomic concepts as well as evaluations of their application to programs of the federal government in PPBS. Papers on discount rates, shadow prices, the evaluation of externalities, and the experience of PPBS in defense and domestic applications give the work depth and breadth, though political scientists may find that inadequate attention has been given to noneconomic issues.

Organizationally, the book follows a useful sequential pattern. The question of how to delineate the parameters of the public sector by economic analysis is approached from different vantage points and provides a stimulating, though heavily theoretical, beginning. Peter O. Steiner examines the nature of public goods and the public interest, stressing the multiple factors which contribute to their determination and the limitations of using efficiency alone as a criterion. His discussion establishes a launching point for papers on issues involved in choosing market versus nonmarket allocations of resources (Kenneth J. Arrow), alternative governmental actions for dealing with problems caused by technological externalities (Otto A. Davis and Morton I. Kamien), the importance of uncertainty as a cause of market imperfections and a determinant of appropriate government action (Richard Zeckhauser), and a paper by Burton A. Weisbrod on the pervasive impact of most governmental economic actions on the distribution of income.

Of special note among the discussions of institutional considerations in determining effective public policies through the expenditure route are Charles L. Schultze's treatment of incentives, penalties, and rewards and Murray L. Weidenbaum's analysis of numerous formal restraints in the current federal budgetary process. Schultze lucidly examines the role and potentialities of incentives in the several stages of policy making, the types which hold most promise, and the theoretical and political constraints which determine their relevancy. Weidenbaum perceptively indicates how federal trust funds, permanent and indefinite appropriations, fixed earnings, and construction or other long-term projects have provided major stumbling blocks to rational decision making.

The section devoted to selected analytical problems in policy determinations is of generally high quality and a pertinent prelude to the discussion of PPBS. Two papers merit particular attention: William J. Baumol's discussion of discount rates and the development of principles for determining them, and Julius Margolis's discussion of shadow prices for incorrect or nonexistent market values.

Papers in the final sections on PPBS in the federal government are especially valuable.

Certainly no single methodological advance in federal decision making has had more impact and been surrounded by more controversy than the introduction of this conceptual system. These chapters are mostly by authors who have been deeply involved in implementing the PPBS approach, and their insights reveal the causes of both its failures and successes.

Unfortunately, the implication that PPBS heralded the beginning of "analysis" or "rational analysis" in federal budgeting and decision making seems to pervade a number of the papers. This, of course, is a somewhat circumscribed view of administrative history, since the basic concepts of PPBS—planning, programming, defining objectives, cost-benefit analysis, multi-year financial plans—did not originate in the mid-1950s or '60s. They were recognized in the theory and practice of federal budgeting and decision-making during the 1940s and 1950s. PPBS performed the major feat of combining these elements into a single rational system, emphasizing them, extending their scope, altering and structuring their presentation, establishing a standardized language, creating an educational system to supply technicians, introducing some newly refined tools of analysis, and imposing a rigorous discipline upon the process. It is stretching to represent it as more. Perhaps greater balance might have been given to the discussion of PPBS in the federal government had papers been included by scholars and officials who participated in the work of the Bureau of the Budget and the more advanced federal agencies before the advent of PPBS.

To some extent the need for greater historical and political perspective is provided by economists with governmental experience. Alice M. Rivlin, in examining the application of PPBS to the Department of Health, Education, and Welfare, places PPBS in a real-world perspective, recognizing its strengths and potential for achieving more quantitative analysis and reason in decision making, but at the same time realizing the constraints imposed both by politics and individual value systems. It is a refreshingly candid discussion which helps to dispel some of the more assertive claims of accomplishment elsewhere in the book.

Political scientist Aaron Wildavsky provides a colorful analysis in "Rescuing Policy Analysis from PPBS," and probes an obvious weakness of the more passionate advocates of PPBS—a limited political perspective. But, in all fairness, he is at times inclined to overstatement, as in the following comment: "PPBS must be tremendously inefficient. It resembles nothing so much as a Rube Goldberg apparatus in which the operations performed bear little relation to the output achieved. The data inputs into PPBS are huge and its policy output is tiny . . ." (p. 469).

Space does not permit an adequate critique of the many fine papers on the strengths and limitations of PPBS in enhancing budgeting and decision-making. But those on PPBS and Congress by Senator William Proxmire and on PPBS's status and future by Assistant Director of the Budget, Jack W. Carlson, as well as the lucid appraisal and analysis of resource allocations in health by Robert N. Grosse all provide much insight and substance.

All in all, *Public Expenditures and Policy Analysis* is a most useful core work for courses of study in public finance, budgeting, and decision making.

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The Seniority System in Congress. By Barbara Hinckley. (Bloomington: Indiana University Press. 1971. Pp. 146. $5.95.)

Barbara Hinckley sets out to show that the seniority system, instead of being a villain, and a bastard to boot, is a legitimate offshoot of the American political system, not responsible for all governmental ills, and possibly even possessed of some heroic qualities. Her goal is modest, as are her expectations, for she doubts that the study will have much effect on the conventional wisdom concerning seniority. Clearly, the book will be most convincing to those who are already convinced. I believe, however, that even the skeptical will have to admit that *The Seniority System in Congress* is a well written, ingeniously researched, and carefully organized monograph.

Prof. Hinckley focusses on the claims that the seniority system gives leadership in Congress to aged, conservative representatives from one-party and rural areas who have a strong antiparty and anti-President bias.

Her research concentrates on the ten postwar congresses that served between 1947 and 1966. She frequently uses a peel-away technique which I found both interesting and valuable. For example, when she is studying the impact of the seniority system on the share of committee posts going to House members from rural districts, she lists, in the extreme left hand column of a table, the number of rural and urban congressional districts in the hands of Democrats in 1961; in the extreme right hand column, the number of committee chairmen from rural and urban districts in that year. In between, she lists, in descending order, the number of districts (1) that had been held by the same party, (2) that had been held by the incumbent, and (3) that had had members on the same committee for at least the preceding decade. Her conclusion is that "none of these factors linked to the seniority system—even in combination—is sufficient to explain the rural predominance in chairmanships" (p. 62).

At the end of the study, she draws a similar but broader conclusion:

> The effect of the seniority system is limited because its requirement of continuous service in House or Senate can be met by a majority of the congressmen. . . . Indeed, there are more congressmen qualified by long congressional service for committee chairs than there are chairs to be filled; hence factors other than congressional seniority, such as original committee assignments, and subsequent reassignments, can and do influence the selection of chairmen (pp. 108–109).

She does suggest that changing the system would be "something on the order of redirecting ocean currents," (p. ix) and that the status quo may have some advantages: "Power centralized in the hands of party leaders could be more easily controlled from the outside than power dispersed among many centers. The seniority system . . . strengthens the Congress in its well-known desire to be independent of the Presidency" (p. 112). Then she goes on to describe it as "a profoundly conservative institution—not because it biases the kind of leaders selected, but because it reinforces the conservatism already present in Congress" (p. 113).

I must admit that I wish Mrs. Hinckley had been a little more positive in her last chapter. Is it necessarily conservative to have a seniority system which reinforces separation of powers and, in so doing, keeps the President somewhat closer to being the first among equals rather than monarch among courtiers? I would think that faculty members, who enjoy the benefits of a tenure system, would be able to understand the benefits to the American government of a not too dissimilar seniority system.

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The Years of MacArthur. Vol. 1: 1880–1941. By D. Clayton James. (Boston: Houghton Mifflin Company, 1970. Pp. 740. $12.50.)

The first volume of a two-volume life of General Douglas MacArthur being written by D. Clayton James, a historian at the University of Mississippi, is a major achievement in its own right. It is the first scholarly biography of this paradoxical military figure, based on exhaustive examination of documents and interviews held with high-ranking military officers, including General Eisenhower.

In part, it is the biography of a family, since Douglas MacArthur represented the third generation of public figures. The grandfather was sufficiently active in politics to become a lieutenant governor and a federal judge. The father, General Arthur MacArthur, began a precocious military career in the Civil War. After many years of professional military life, he became Military Governor in the Philippines, and while achieving the rank of Lieutenant General, was probably cut off from the highest army position of Chief of Staff because of a feud with William Howard Taft. The fierce pride of a protecting mother helped Douglas become the noble patrician—a role the author sees as far more characteristic of Douglas the military man than was the role of military despot.

Since Professor James makes the book "in some respects, a story of the development of the modern American Army" (p. viii), it is valuable for its description of West Point during General MacArthur's years as a cadet. This description serves as background for the very striking chapter on General MacArthur's term as a young superintendent after World War I. The author asserts that "he, more than any other man, led West Point across the threshold into the rapidly changing world of military education, . . . one of his most important contributions to the development of the modern Army" (p. 294).

Since I was both a G.I. in MacArthur's theater during World War II and a junior officer in his headquarters, I found this first volume tantalizing because it takes MacArthur only as far as Pearl Harbor. Moreover, the author himself states his belief that General MacArthur "a century hence will be most appreciated for his role as an administrator, rather than as a warrior" (p. viii), and that the most important period of his life was 1946–1950, which will be covered in the second volume. The account of MacArthur's service as a combat general in France during World War I, however, foreshadows some of the characteristics he displayed in the Pacific War.

One of the paradoxes of General MacArthur's military career is on the one hand his ability to evoke the fierce loyalty of subordinates and on the other his phenomenal lack of judgment in certain basic areas. His ability to promote loyalty is seen in his years as public relations officer under Secretary Newton D. Baker before going off to France, and later as Chief of Staff, fighting to regain a viable force after the lean years following World War I. His lack of judgment as Chief of Staff is apparent in his approval of plans for military mobilization that were found unrealistic by his successor and in his failure to foresee the need for mechanization. Furthermore, when MacArthur became Field Marshal under President Quezon in the Philippines, he grossly overestimated the defense program, and he underestimated the Japanese military capacity and did not anticipate the war. His Philippine plans for a citizen's army were criticized by some military men and journalists, both in the Philippines and in Washington, as unrealistic. Professor James accomplishes a great deal in setting up this record of MacArthur's achievements and shortcomings.

Valuable sections of the book cover MacArthur's term as Chief of Staff, his contribution to a program of reorganization of the Army, his role in the court-martial trial of General William Mitchell, and the notorious routing of the Bonus Army. As war clouds became unmistakable in the Pacific, MacArthur got the combined command by virtue both of his lifelong concern with that area and of some personal maneuvering. The Philippine Army was not remotely prepared for the combat which it would face, and for this the author places blame squarely upon MacArthur for "his over-confidence and unjustified optimism as to the abilities of himself, his staff, and the untried Filipino soldiers. . . ." (p. 609) in the context of support from Washington that was too little and too late.

Professor James, although meticulous in his scholarship, does not shun judgments. The force of these judgments is not always clear, because sometimes they do not flow clearly from the material he presents, or they lose strength from the manner in which they are presented. All in all, however, this is a distinguished beginning, and Volume II will be eagerly awaited.

Dale Pontius

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Felix Frankfurter on the Supreme Court: Extrajudicial Essays on the Court and Constitution. Edited by Philip B. Kurland. (Cambridge: The Belknap Press of Harvard University Press, 1970. Pp. x, 572. $12.50.)

The verdict on Felix Frankfurter the Justice is not yet in, and there is every reason for believing that the jury will be deadlocked for some time to come. Result-oriented detractors will no doubt continue to stress that in twenty-two years on the Court he authored remarkably few enduring "landmark" constitutional decisions: after all, *Gobitis, Wolf,* and *Colegrove,* for example, failed to survive even his own tenure on the Court. Loyal followers, on the other hand, will just as assuredly persist in avowing that the measure of individual greatness is neither the number of Opinions of the Court that make the casebooks nor the number of dissents that eventually become the Law of the Land. Felix Frankfurter's greatness, the acolytes will continue to argue, is to be found in his craftsmanlike opinions, which set a principled and scholarly tone for constitutional decision making: he clarified issues for a sometimes stumbling and befuddled Court; illuminated the past and probed the future for a sometimes myopic Court; and, most important, constantly reminded an increasingly bold Court of its own limitations.

Whatever the judgment on the judicial tenure of Justice Frankfurter, his extra-judicial comments on the Constitution and the Court, meticulously and thoughtfully edited by Professor Kurland, lend weighty support to one proposition: Felix Frankfurter, off the bench, was the most astute student of the Supreme Court in the first six decades of this century. Such a bold judgment requires some explanation; there are of course other worthy claimants—each of whom was in some respects Frankfurter's peer or better. To confine the roster to those no longer among us, Edward S. Corwin, Charles Grove Haines, Zachariah Chaffee, and Mark DeWolf Howe were at least his equal in mapping the highways and byways of constitutional development. And it is difficult to imagine more skillful dissectors of cases or exposers of faulty reasoning than T. R. Powell or Henry M. Hart. Yet on balance, Frankfurter possessed a versatility and catholicity of knowledge which none of the others could match. He was the one Court-watcher who was at once a legal scholar, historian, philosopher, and a close—and active—student of the social and political scene. He was the only scholar to bring such a background to the study of that which he so adored—the Constitution and its principal expounder, the Supreme Court. Whether writing for the *Harvard Law Review*, the *New Republic*, or a convocation of notables, he managed with characteristic clarity and grace to bring fresh insight to the topic at hand.

Professor Kurland's rich collection of off-the-bench Frankfurter has much to tell about Justice Frankfurter's performance on the bench. First, although many of the Justice's cruder critics have accused him of changing his views (i.e., of abandoning "liberalism") when he donned the robe, anyone familiar with his early writings can hardly claim surprise at his opinions in the area of individual rights. A 1925 article presaged his future stand in *The Flag Salute Cases, Dennis, Feiner* and a host of other cases involving Jehovah's Witnesses, Communists, and sundry street-corner rabble-rousers. In a *New Republic* essay entitled "Can the Supreme Court Guarantee Toleration?" he applauded the Court's holdings in *Pierce* v. *Society of Sisters* and *Meyer* v. *Nebraska;* at the same time he made it equally clear that striking down illiberal legislation in the name of the Constitution was hardly distinguishable from vetoing minimum wage legislation, and "that the real battles of liberalism are not won in the Supreme Court." Similarly, a 1930 piece previewed his celebrated *Adamson* concurrence. Rejoicing over the recent decision in the *Scottsboro* case, he observed that the fourteenth amendment "is not the basis of a uniform code of criminal procedure federally imposed . . . [i]n no sense is the Supreme Court a general tribunal for the correction of criminal error." What the Court "will not suffer [is] . . . 'judicial murder.' " Justice Frankfurter practiced pretty much what Professor Frankfurter preached.

Second, if the early writings suggest that the essential commitment to judicial "self-restraint" was well established by the time Frankfurter was elevated to the Court, they also provide a clue to the source of his agonizing and the ambivalence which characterized so many of his most notable opinions. One possible reason that pat solutions did not flow facilely from the Frankfurter pen was that, observant as he was, he did not fully realize that his two acknowledged mentors and heroes, Justices Holmes and Brandeis, held rather dissimilar attitudes about the nature of law and the judicial process: Holmes was a detached and skeptical philosopher who eschewed the popular press in favor of the classics; Brandeis, a committed and policy-oriented statesman who was concerned with what he labeled in quite un-Holmesian language "the concrete problems of practical justice." As a law professor or publicist between World War I and the Second New Deal it was relatively easy to maintain simultaneous allegiance to both, since their approaches usually led to the same result when it came to concrete cases. As a Justice in the 1940's and 1950's, after the Court's focus had shifted from economic regulation to civil rights and liberties, it was difficult to be *both* Holmes and Brandeis. The skeptical Holmes might well have voted to uphold the state in the *Flag Salute Cases;* his dissent in *Meyer* v. *Nebraska* dramatizes the extent to which he would defer to the judgment of the political branches. On the other hand, it is difficult to see Brandeis, who was with the majority in *Meyer*, in such a role; as Professor Louis J. Jaffe has brilliantly argued ("Was Brandeis an Activist? The Search for Intermediate Premises," 80 Harvard Law Review 986 [March, 1967]), Brandeis *was* an "activist" and, as his concurrence in *Whitney* and dissent in *Olmstead* evidence, he was forthrightly prepared to say no—and in sweeping language. (While Holmes joined Brandeis' opinion in *Whitney*, the language is pure Brandeis; and it is significant that Holmes elected to write a separate dissent in *Olmstead.*) Nowhere is the tension between the philosophies of Holmes and Brandeis more evident than in the opinions of Felix Frankfurter. Indeed, one could argue that there were perhaps two Frankfurters—the Holmesian Frankfurter of *Gobitis* and the Brandeisian Frankfurter of *Sweezy.*

Finally, his essays about, and tributes to others who had the privilege of sitting on the Supreme Court suggest that Felix Frankfurter labored under the burden of being the most self-conscious Justice ever to sit on the Court. The evidence abounds. He was notably preoccupied with measures of greatness, judicial style, and the art of opinion writing. Thus some argue, usually by implication, that Frankfurter was so engrossed with the critical judgments of posterity that his own opinions all too often sacrificed substance for style. Such a view is wide of the mark. Frankfurter never lost sight of substance; rather he saw clearly that style and substance were inextricably related. Law was of course not literature; but opinion writing involved the art of exposition, and he believed that the great Justices possessed the ability to set forth their ideas with compelling force and clarity. If Justice Frankfurter's opinions appear self-conscious, overwritten, or unnecessarily academic, it was because he was the Justice most painfully aware of the perennial and subtle paradoxes of constitutional adjudication.

Professor Kurland's skillfully edited compilation of Felix Frankfurter's extrajudicial essays is not just another collection of legal papers. Rather it is a fitting and revealing testimony to the persistently self-demanding, complex, and subtle qualities of mind that Felix Frankfurter brought both to the study of the Supreme Court and to the Court itself. Whether the essays reveal more about the Court or about their author is an intriguing question. Their merit is beyond dispute.

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The Wartime Journals of Charles A. Lindbergh. By Charles A. Lindbergh. (New York: Harcourt Brace Jovanovich, Inc., 1970. Pp. xx, 1038. $12.95.)

While living in England in November 1937 Charles A. Lindbergh, the famed aviator and national hero, felt that Europe stood on the verge of war and that as a close observer on the scene and consultant to the American government on European aviation he was himself taking an important part in the events that were forming "one of the great crises of world history." So he decided to do what he had never done before, to keep a daily log of his activities and thoughts. Although he had no intention at the time he wrote them of publishing his journals, he believed they could be valuable because aviation had become a possibly decisive element in warfare, a development he was in a unique position to appraise because he had access to political and military leaders in the great powers of Europe. Later, when he opposed American intervention in the Second World War, becoming a leading spokesman for the America First Committee, his journal keeping acquired another purpose, to serve as his personal record of the battle against intervention.

With only one gap of about a year, Lindbergh kept up his log books until the summer of 1945 when he visited wartorn Europe to examine once more what remained of German aviation. By that time his purposes in maintaining his daily diaries had been achieved and he wanted to devote himself fully to writing another book, *The Spirit of St. Louis*. Twenty-five years later, still smarting from a feeling of having been persecuted for his political activities during the debate over intervention, he decided to publish the journals so that posterity could view the record as he saw it. These journals, reduced by a third from the entries in the original manuscript books but intact in the essentials, comprise that record, an account of seven important years in Lindbergh's life. Believing that men can profit from past experience, he also had a utilitarian purpose in publication. He hoped his journals would "help clarify issues and conditions of the past and thereby contribute to understanding issues and conditions of the present and the future."

What emerges from this big, rambling book is the picture of a troubled, introspective, sensitive, articulate conservative man of inflexible opinions and of often naive political and social attitudes. As an isolationist, Lindbergh worked to keep the United States out of a war that he thought threatened the survival of Western civilization. This is why in 1939 he gave up the privacy he and his family deeply cherished to become a political activist and endure the abuse that politics brings. He opposed America's entrance into the Second World War not because he considered war senseless, but because he wanted his country to fight "on the *right* side of an intelligent war!" Even though he allied himself with pacifists in his antiwar activities, he disliked pacifism.

Nordics elicited Lindbergh's admiration, and it pained him to see "our own northern people" fighting each other. This theme, a form of racism which sees Nordics as somehow superior to other peoples, weaves itself through the journals. "I cannot help liking the Germans," he wrote in the opening pages. "They are like our own people" (p. 5). His admiration for the Germans led him to believe that he had found more "personal freedom" in Nazi Germany than in any other European country, or the United States. Germany, in his view, stood as a bulwark against the hordes of Asia and the communism he detested. "A Russian-dominated Europe would," he felt, ". . . be far worse than a German-dominated Europe" (p. 599).

Along with the Soviets, Lindbergh disliked the English (in whom he had lost confidence), Jews, and Franklin D. Roosevelt. "England," he wrote, "is a country composed of a great mass of slow, somewhat stupid and indifferent people, and a small group of geniuses" (p. 22). He thought that British and Jewish propaganda was pushing the United States into the Second World War, and that Jewish interests in the United States, through influence in the press, radio, and motion pictures, were behind much prowar sentiment. He distrusted Roosevelt, believing him something of a dictator who was maneuvering the country into war. This view has since become standard among revisionist historians of the Second World War.

In his antiwar speechmaking Lindbergh encountered the usual restrictions that even a democracy places on unpopular dissenters. His protest over the difficulties placed in the way of the America First Committee when it sought to buy radio time to present its views has a contemporary ring. "It is a fine state of affairs," he properly complained, "if the question of war and peace cannot be debated before the American people because it is a 'controversial issue'!" (p. 294).

Lindbergh also reveals a sensitivity toward a foe that stands in contrast to his blindness to the brutality of Nazism. It is hypocritical, he rightly points out, to condemn the Japanese as cruel and barbaric, and then commit atrocities against them while maintaining the credo that America stands for "all that is 'good' and 'right' and civilized" (p. 880).

Despite such refreshing insights, Lindbergh's conservative views on international politics have not changed much in a quarter of a century. His anticommunism appears as rigid as ever. He laments the demise of the British and

French empires, implying that the temporary ascendancy of the West over the rest of the world could have been and should have been permanent. His is the attitude of a racial elitist, based on the assumption that the dominance of the Western "civilized" whites over the colored peoples of the world is a good and healthy thing.

Regardless of the rightness or wrongness of Lindbergh's views on world affairs, for political scientists and historians this book is a basic document for the study of conservatism and isolationism in twentieth-century America. Although the main parts of the picture it reveals are well known, it does give fresh details about the life and mind of a sincere, conservative isolationist, and on the inner workings, the tensions, and the problems of the America First Committee. It is most valuable, therefore, as a sourcebook.

ALEXANDER DECONDE

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Grand Old Party: Political Structure in the Gilded Age, 1880–1896. By Robert D. Marcus (New York: Oxford University Press, 1971. Pp. 323. $7.95.)

The Money Machines: The Breakdown and Reform of Governmental and Party Finance in the North, 1860–1920. By Clifton K. Yearley. (Albany: State University of New York Press, 1970. Pp. 377. $12.00.)

Virtually everyone is agreed that the 1890's mark a basic watershed in American political history. But the question of precisely what changed and why is subject to much dispute. Dean Burnham has documented the sharp changes in political participation, and E. E. Schattschneider has shown the changes in political competition and public policy. It was in 1896 that McKinley and urban America decisively defeated Bryan and inaugurated a Republican era. The 1890's also saw the emergence of the "solid" Democratic South, based on final exclusion of Negro voters, and the emergence of the United States as a major figure in world diplomacy (and imperialism). After 1896, the House of Representatives began to display its distinctive modern pattern of high rates of incumbent reelection and consequent increased length of service of members. After the assassination of McKinley, the country could look to its first "modern" President and read of his exploits in the new mass journalism of the era. Only the Senate and Supreme Court seem to have escaped major change in the decade 1892–1902.

The two books reviewed here both deal with change in the area of political parties, one emphasizing party organization and the other party finance. Robert D. Marcus set out to test the hypothesis of Robert Weibe (who directed the dissertation from which the book emerged) that the late 19th century was an era of increasing bureaucratization, centralization, and "search for order." Marcus is disarmingly frank in announcing that at least for the Republican Party of 1880–1896, he found hardly a shred of evidence to support the hypothesis. Obviously the generalization may well hold for many sectors of American life—including the House and Senate—but *not* for national political parties (not even in the days of Mark Hanna, who was more the product of events than their master).

Fortunately, nothing is lost save a rather casual hypothesis. Marcus goes on to give us an excellent analysis, centered around the quadrennial conventions and elections, of the national GOP from 1880 to 1896. He writes exceedingly well and is obviously a master of his sources (primarily archival, though he is very much aware of recent quantitative political history). Perhaps to atone for the lost hypothesis, he closes with an extremely perceptive chapter on the changed role of presidential elections after 1896. This analysis involves some shrewd speculation on the reduced scope of parties and elections after 1896, such that *legitimation* of policy continued to be based on presidential elections, but most foreign and much domestic policy was effectively determined by various "private governments" (quasi-judicial commissions, pressure groups, and government bureaucracies). This hypothesis makes the 1896 realignment relevant to Gabriel Kolko's revisionist interpretation of the Progressive Era.

Clifton Yearley's *The Money Machines* tackles a more ambitious and much neglected subject—the relation of governmental and political party finance. The author has immersed himself deeply in economic, historical, and political materials for the period 1860–1920, but somehow he fails to pull the material together in any comprehensive or easily understood way. There is useful material here and there on the nature of middle-class "reform," the workings of local "machines," and competing theories of taxation. But the systematic view of the way in which parties—and local governments—went about raising funds, and the relations between parties and local governments somehow never jells. As the title might suggest, the author slips too easily into metaphor and never achieves a systemic overview.

Some scholars—one thinks of Dean Burnham and Grant McConnell—tend to write of the politics of pre-1896 America as a sort of "golden age" of high participation and democratic groups. Neither Marcus nor Yearley concentrates on the sordid side, but they clearly do *not* portray the political scene of, say, 1874–1894 as any Garden of Eden. Indeed, a case could be made for viewing this period as an era of pathological politics. Partisanship was rampant, election administration was a joke, patronage and assessments were the rule, pension and mail fraud were widespread; and if particiation was high, so was the investment in means—both fair and foul—for mobilizing both the faithful and the apathetic. Interestingly, the resulting powerful party organizations differed hardly at all when it came to public policy (other than returning Confederate battle flags). If bosses and machines had their latent functions, they also had manifest disadvantages.

The era of late 19th-century politics is not an easy one to make sense of, but it *is* important as the foundation from which our 20th-century politics has grown. Both of these books make modest contributions to that goal—Marcus by his balanced overview of party machinery, and Yearley by the range of his scattered sources. For a more complete understanding we will have to go beyond metaphors of "money machines" and pat phrases, or continue to hope that the facts will speak for themselves. They seldom do.

HUGH DOUGLAS PRICE

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Political Socialization and Educational Climates: A Study of Two School Districts. By Richard M. Merelman. (New York: Holt, Rinehart and Winston, Inc., 1971. Pp. 268. $3.95.)

Unlike much research in political socialization, Merelman's is clearly and overtly policy oriented. In this book, he is mainly concerned with the school as an agent of political socialization (which he dimensionalizes as democratic attitudes, political knowledge, partisanship, and desire to participate in politics) and with "whether the aspects of education that occupy most of the community attention, such as the quality of instruction and the character of the teaching staff, have effects on the political socialization of students" (p. 8). The impact of these independent variables on political socialization is selected for study because they may be influenced by educational policy makers, and because they are highly "amenable to policy innovation or change" (p. 8).

The book is an important addition to the existing literature on political socialization: it covers a broad range of hypotheses, breaks new ground in its methodological approach, and explores the normative implications of its empirical findings. But in touching so many topics of interest, one cannot treat any single area of concern very thoroughly. Furthermore, innovative methodological approaches can sometimes be questioned because they are new and insufficiently explored. Finally, the normative implications of data are sometimes difficult to discern, because they are often dependent on ideological interpretation.

Sixth-, ninth- and twelfth-graders in two Los Angeles school districts filled out questionnaires measuring commitment to several dimensions of democratic norms (support for the vote, freedom of speech, majority rule, minority rights, sense of civic obligation, rules of the game, importance of elections, opportunity to enter politics, and political efficacy); as well as level of political knowledge, support for domestic liberalism, party identification, anticipated level of political activity; and such nonpolitical variables as participation in school organizations, mass media consumption, sex, social class, and school achievement. Merelman selected these two districts because, while they were similar in some respects, the wealthier one "supported education poorly" (p. 57) while the other was more committed to quality education (some examples of quality education are: greater financial outlay, smaller size of classes, more educational innovativeness). While a positive relationship between "district financial support for education, teacher quality, and the development of democratic values in students" (p. 21) was initially predicted, the results of the study surprisingly failed to confirm this hypothesis. The school that was more committed to quality did not produce "superior" political socialization of its students. It is possible that some of the demographic characteristics of the districts (for example the district committed to quality is 30 per cent Jewish and somewhat more politically liberal and Democratic than the other) confounded the manipulation of district commitment to quality education. In any case, the author is refreshingly open in pointing out both the strong points and the problems in his research design and analysis. He leaves his reader with few unanswered questions.

Merelman groups the data from his two districts together to analyze the effects of conditioning agents on dependent variables. He notes that "the party system exerts little attraction" (p. 88) and that "participation motivation disintegrates" as age increases (pp. 88–89). Low acceptance of "democracy" even in these two white middle-class school districts is not a rare phenomenon. Several other studies of political socialization made after 1967 have reported similar findings. These results reconfirm the marked differences in political orientations noticeable between children studied in recent years and those polled by political socialization specialists in the late 1950s and early 1960s.

A sample of teachers from the two districts answered questions concerning their feelings about the profession, their career patterns, their level of morale, their satisfaction with teaching, their relationships to colleagues, students, and administrators, their self-evaluated performance in the classroom, their personal political attitudes, and so on. Few important differences were found on these variables between the teachers of the district which had a high commitment to quality education and the teachers of the district where quality education was a less important priority. This finding casts doubt on a district's differential ability to recruit superior staff even when community support and administrative commitment for such a goal is high. The cleavage was not between districts, but rather between teachers at different levels and in certain subject matter areas. Thus the secondary school teachers and the social studies teachers in both districts taken together were found to differ from the other teachers on many variables.

With respect to methodological innovation, instead of using a specific set of items to tap democratic structure, Merelman factor analyzes, for each grade level, a large pool of items in order to determine empirically what structure exists in the youngsters' response patterns. The results of this factor analysis are, in some cases, subsequently used to adjust which items will be used to construct the final scales of Support for Democracy. The disadvantage of this procedure is that while theoretically the final scales tap the same dimension, in some cases where required by the factor analysis, they are constructed of different items. This scaling technique may call into question comparisons between grades, because the same scale is constructed of different items at each grade level.

Merelman is also original in his attempt to link macro-level variables (district characteristics) with micro-level behavior and attitudes. It is difficult to evaluate definitively the appropriateness of this procedure. One is more willing to accept it when the macro-level variable has a clear impact on the individual student, e.g., in the case of size of class or district expenditure per student. But aggregate comparisons which are undertaken in order to make generalizations about individual behavior—for example, the attempt to measure teacher influence on students—seem more dubious. Hess and Torney, in a similar analysis, try to determine if students come to share the political attitudes of their teachers. They use the students' own teachers as a reference point. Merelman, on the other hand, compares a sample of students, not with their own teachers, but with an independent random sample of teachers from the district (pp. 162–164, and p. 235). This is clearly a less powerful way to make such a comparison.

Merelman feels obligated to relate his empirical data to his normative concerns (p. 210). While normative decisions enter his research at many points, including operationalization of variables like democratic socialization, this commitment culminates in his concluding chapter with recommendations to psychologists, educators, and policy makers. These recommendations include such controversial measures as evaluations of teachers based on achievement tests given to students before and after they have studied with a particular teacher. Unfortunately some of the recommendations are not closely linked to the data reported in the book. For example, policy makers are told that more funds must be made available for public education. Yet the first part of the book shows that higher expenditures in one district failed to lead to superior performance on the part of its students and did not produce the markedly distinctive staff one would have expected.

In conclusion one must note Merelman's tremendous interdisciplinary command of the literature. He carefully relates to his own work pertinent research from psychology, sociology, and education. In addition, perhaps because of the largely negative results of the survey reported in this book, some of the most interesting and useful material is Merelman's description of the dynamics of education and teaching in a large U.S. city. His understanding of administrator and teacher motivations and his insights into district educational politics alone make this book well worth reading.

PAULINE MARIE VAILLANCOURT
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Kennedy Justice. By Victor S. Navasky (N.Y.: Atheneum, 1971. Pp. xx, 482. $10.00.)

It is difficult for the reader of the daily newspapers to get the sense that presidencies are actually about the politics of capturing—or learning to be captured by—the routines of government, and then making the government run. It is doubly difficult for the student of the Kennedy Presidency to get this sense, for reasons that are well enough known. We can be grateful to Victor Navasky for not being so beguiled by the Kennedy glamor as to have produced yet another misleading and redundant tribute to Camelot. Instead he has given us a detailed chronicle and serious assessment of the way in which Robert Kennedy operated the Justice Department—the best discussion I know of that deals with national politics from the perspective of a cabinet officer.

Navasky's study is generally instructive precisely because, as he points out, the Kennedy Attorney Generalship must constitute a limiting case; surely no head of a department has ever enjoyed better or more assured access to and support from a President than Robert Kennedy did from his brother. Thus Kennedy was, as Navasky says, the maximum Attorney General, at least so far as constraints and difficulties from above are concerned.

Yet, as we rapidly discover, this is by no means enough to assure maximum performance on all fronts. Rather than a clearcut hierarchical Justice Department, in which the maximum Attorney General automatically gets the outcomes he wants, Navasky (courtesy of Max Weber) proposes the metaphor of a Justice Department which embodies an interaction among three giant forces: the code of the Kennedys, legitimized by charisma, activist, result-oriented, red-tape cutting, and loyal to persons; the code of the Ivy League Gentleman, embodied in Kennedy's top appointees (Burke Marshall, Nicholas Katzenbach, Byron White, Louis Oberdorfer, *et. al.*), legitimized by tradition, proceduralist, oriented to civil liberties, limited government, due process, federalism, and letting the chips fall; and the code of the FBI, legitimized by the existence of files, secretive, routine-oriented, self-protective, and loyal to habit.

The first and in some respects the most significant part of the book describes the way in which Robert Kennedy came to terms with J. Edgar Hoover, the maximum bureau chief. Since the FBI employs almost half of the people in the Justice Department, the nature of this bargain was bound to have enormous impact upon what Kennedy and his appointees could do in their time in office. Navasky concludes that the FBI imposed considerable constraint upon Kennedy Justice, especially in the field of civil rights, where the Bureau was not quick to adapt its priorities to those of the Attorney General. Navasky suggests one highly plausible reason for Bureau-Department tension in this area. Increasingly in the early 1960s, leaders of the Department came to believe that it was necessary to ride herd on local law enforcement agencies throughout the south in order to provide minimal levels of protection to civil rights workers. Navasky surmises that part of the FBI's reluctance to participate in this effort arose from long-standing ties between the Bureau and local law enforcement agencies. To disrupt these ties in order to meet an extraordinary situation might have jeopordized the ongoing routines in which the Bureau was productively enmeshed with local people.

The identification, examination and diagnosis of dilemmas such as these are central to at least one branch of political science. Navasky shows how and why Attorney General Kennedy challenged, circumvented, and gave hostages to the FBI and to J. Edgar Hoover. This discussion contributes helpfully to our understanding, otherwise seriously deficient, of the politics of running the great departments of our national government.

Other sections of *Kennedy Justice* take up other facets of the Attorney General's world. Herewith, a small sample: A long section on federalism, slightly marred by an excess of second-guessing by Navasky, explores the role of the Justice Department in the integration of southern universities. Here, Navasky argues that the code of the Ivy League Gentleman, and the theory of federalism to which it gave rise, hemmed Kennedy in too much. Another chapter argues that the eagerness of the American Bar Association to legitimize its participation in the selection of federal judges, coupled with serious lapses in the Justice Department's attentiveness to the credentials of nominees, produced some very bad judicial appointments in the South. Navasky only takes up in detail, however, the handful of notorious cases. His legal training (though a practicing journalist, Navasky is a graduate of Yale Law School) might have led him to a more ambitious and more enlightening task: examining the performance of a larger sample of Kennedy judicial appointees in order to see if the Justice Department's alleged pattern of semi-indifference to the quality of the bench was sustained in areas other than the South, and for issues other than segregation of public facilities.

While Attorney General Kennedy's light touch with his bureaus is held to have been relatively ineffective in dealing with J. Edgar Hoover, Navasky thinks Kennedy struck just the right note with Archibald Cox, the distinguished and independent Solicitor General. Navasky gives a detailed and intriguing account of the complex negotiations in and around the Solicitor General's office surrounding the decision to file an *amicus* brief for the government in one of the reapportionment cases. My view is that he is a little too sure that the outcome was the right one.

The *pièce de résistance* of the book is the last chapter, on the campaign to get James Hoffa, showing Kennedy Justice at its least inhibited, as the early chapters show it at its most inhibited. Navasky raises some proper questions about this project, and I think quite eloquently puts the case for a more constrained Attorney General to counterpoise the case he makes earlier, in regard to civil rights, for a less constrained one.

This is not a flawless book. It is a bit repetitious, and once in a while a bit smug. I found a few small inaccuracies in it. On the whole, however, I think *Kennedy Justice* is thoughtfully, artfully, and carefully done. There are a good many lessons in it for students of the craft of governing. Some of these are hard lessons, since they force us to consider what we can reasonably expect from leaders of government less advantageously placed than the maximum Attorney General.

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The Texas Political System. By Dan Nimmo and William E. Oden. (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1971. Pp. 166. $5.95.)

Texas Politics: An Introduction. By James Anderson, Richard Murray and Edward Farley. (New York: Harper & Row, Publishers, Inc., 1971. Pp. 327. $3.95.)

Whether the slim volume by Nimmo and Oden finds favor with today's political scientist may depend upon whether the reader regards systems analysis as *the legitimate* frame of reference. This book is assertedly "the first application of a systems approach to the study of any state government" (blurb). The supports and demands of Texas politics (inputs) and the conversion of stress-producing demands into authoritative allocations of benefits and deprivations (outputs) are to be identified and compared with input-output data from other state political systems in the United States. Systems analysis functions as a fairly clearcut paradigmatic framework which facilitates the perception of political phenomena. The emphasis is on identification and analysis of the uniformities of data and functions. Systems analysis performs very well the duty of identification, but it is not yet clear that it facilitates superior handling of problems such as measurement, causal inference, and policy determination, or that it informs about situations and behaviors in a manner which contributes to accuracy in prediction or perception in postdiction.

Nimmo and Oden tell us that beneath the formalities of Texas participatory democracy lies not so much a pluralistic democracy as an elitist reality. They agree with Daniel J. Elazar, *American Federalism*, that the Texas political culture is an amalgam of traditionalistic (a paternalistic and elitist conception of government) and individualistic (government perceived as a marketplace in which policies emerge from the bargaining of individuals and groups acting out of self-interest) rather than a *moralistic* political culture (in which government is perceived as existing in order to promote and maintain a shared public interest—the good society).

Where in this culture do political *demands* arise? According to Nimmo and Oden, they come from strong interest groups which are in the main conservative and grounded in the business community: the oil industry, natural gas, beer, railroads, insurance, trucking, consumer-finance, banking, the Farm Bureau, the engineering lobby, and certain religious bodies. The less prosperous are more "subjects" than "citizens."

Political power resides with a set of loose and shifting coalitions among pressure groups representing the affluent. These groups and their lobbyists grew up with Texas. Economic development preceded or accompanied development of the political structure and shaped that development in its own image. The Texas Constitution of 1876 insulates the decision-makers from popular control. The majority of the adult population are acquiescent or indifferent. The pressure system, say the authors, is the least studied and least understood of all Texas political subsystems, yet it is the "prime filter through which all demands to which policy makers respond must pass" (p. 105).

The authors devote little attention to the often bloody election battles between conservatives and liberals, or to the colorful personalities and political high jinks often featured in these donnybrooks. This neglect is obviously dictated by the emphasis on comparative systems analysis and by the brevity of the work. I cannot resist suggesting, however, that in Texas the demands and supports mobilized by the efforts of tough-skinned conservatives and tenderhearted liberals owe something to individual leadership capacities which deserve attention.

More crucially, the authors apparently conclude that elections and candidates are rather irrelevant in Texas (except perhaps for the legislature) because pressure groups representing the upper economic classes dominate state policies. They also emphasize that the power of the governor is relatively slight. This may be so, but the authors need more evidence to support their findings than is offered in this book. This omission is not entirely the fault of Nimmo and Oden. They point to the dearth of basic data on Texas politics. Lamentably absent are studies on pressure groups and policies, legislative roll calls, identification of the demographic profiles of diverse voting blocs, licensing and other policies of bureaus and commissions, and other important political reference points.

The sketchy outline of the political policy picture in Texas which emerges here is intriguing, but the limitations impose upon the work an exceptionally tentative quality. Unfortunately, there are simply too few conclusions which are not hedged in with qualifications as to the inadequacy of supporting evidence to permit confident comparisons with other states. What is found here is a silhouette, made elegant by tasteful style and by what the authors call a "sophisticated tool of social inquiry."

In sharp contrast to the Nimmo and Oden work, the book by Anderson, Murray, and Farley appears to be typical of the James Bryce emphasis on "facts, facts, facts." The authors' preface affirms a desire to avoid discussion of "reform" and to minimize considerations of "policy." The authors describe in legalistic and structural terms the constitution, administration, judiciary, and local governments, and they use a situational-behavioral framework to discuss parties, voting, elections, interest groups, the legislature, the governor, the courts. A final chapter details the sources and amounts of Texas revenues and the budgetary allocations.

The volume employs quantitative measures in indicating population changes, voting proportions, political party patterns, and the use of the gubernatorial veto. On other occasions the style of the narrative is marked by the use of illustrative examples. Perhaps the best stylistic feature of the volume and its nearest approach to an overview is found in the series of cartoons. My favorite (evidently the publishers liked it too, because it is reproduced on the back cover) shows a bald, potbellied, cigar-smoking individual in conversation with another man. "Jim," says one of them, "suppose the people get a lobby?"

In one respect Anderson, Murray, and Farley offer evidence which amounts to a corrective not only of their own generalization but of one subscribed to by Nimmo and Oden as well—the weakness of the governor. While affirming that he is a weak chief executive relative to the powers of most other state governors, the evidence strongly suggests that the Texas governor exerts a clear and frequent influence on policy, including legislative policy. Evidence shows that the prestige of the office and its occupant reinforces the very tangible power of the veto, including the power of the item veto on appropriations bills. The governor also frequently uses the power to call special sessions of the legislature. In a special session the legislature is constitutionally required to confine its attention to the subject matter specified in the call for the session (i.e., by the Governor). The importance of special sessions is enhanced because the legislature is constitutionally required to meet biennially. Demands of policy make necessary more frequent sessions.

In general I have been struck by the frequent appropriateness with which the data contained in this book might be used to support several major conclusions of the work by Nimmo and Oden. On the other hand, Anderson, Murray, and Farley avoid making these generalizations. Perhaps the confining modesty of their design (for an introductory level book) served as a kind of self-fulfilling prophecy.

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Executives in the American Political System. By John C. Ries. (Belmont, California: Dickenson Publishing Company, Inc., 1969. Pp. 151. $2.95, paper.)

Professor John C. Ries has begun the much needed and difficult task of building a theoretical framework within which the behavior of executives within the American political system may be studied, analyzed, and compared. Ries argues that the literature of the Presidency, governors, and local executives, although frequently rich in detail, adds up to no meaningful whole and that we lack an integrating framework with which to relate executives to political systems. Ries attempts the beginnings of a difficult integration by using relevant concepts from functional-structural analysis, group theory, organizational theory, and role theory. He constructs his framework by (1) identifying and analyzing the principal functions of the executive's role and the key elements within the political system which appear to affect and to structure that role; (2) developing a matrix of political subsystems within which executives may find themselves and evaluating the appropriateness of various executive actions within each subsystem; and (3) identifying and interpreting the factors likely to produce transformations in the executive's role.

Ries states that the primary task of executives at all levels in the American political system is to maintain the system's dynamic equilibrium. To do this, executives must deal with stable demands which continue regardless of the incumbent, changing demands which flow from shifts in political ideology, advances in technology, situational factors such as natural disasters, new participants in the political system, and their own political ideologies and personalities.

The key elements which affect and structure the executive's capacity to perform this task are as follows: (1) the universal functions which the political systems must perform; (2) the political culture which underlies the distribution of power, the scope of governmental action, and the behavior of public officials; (3) the political structure through which power is distributed; (4) the groups within the political system which interact and contend with each other in order to achieve their goals; (5) the diverse outputs of governmental agencies at all levels; and (6) the political resources of the executive.

The core of Ries's work appears to be the matrix of subsystems within which most executives at the local and state governmental levels operate. Drawing upon the elements of the political system developed carefully throughout the first half of his book, Ries constructs his matrix using three major components: (1) performance categories which specify the principal claims made upon governments; (2) power structures which specify the distribution of power among leaders and followers and the degree of convergence of values among leaders; and (3) the executive's political resources. From this, Ries constructs fourteen subsystems and argues that within each subsystem, executives may be classified as weak or strong. Given these subsystems and the resources of the executives within them, it is possible to account for much of an executive's behavior and its appropriateness. It is the system rather than the executive which is the primary determinant, and system role analysis becomes the most useful approach for interpreting executive behavior. Ries summarizes this approach by grouping his fourteen subsystems into seven sets of subsystems and examining each in terms of its frequency, associated policy scopes, decision-making style, political acts, executive resources, and system roles. The system roles include (1) manager—applying skill and expertise; (2) budget-cutter; (3) broker—serving as a go-between; (4) negotiator—participating in the outcome; (5) figurehead—being manipulated by others; and (6) mobilizer—bringing new participants into the process.

Ries does not attempt to place the Presidency within his subsystem matrix because of the lack of knowledge about the distribution of power within the national political system and differences in policy scopes among the national and the state and local governmental levels. He does, however, attempt to identify the roles generally available to the President and concludes that because of the existence of coalitions which seek to use the President, the dominant roles are figurehead and broker and that a negotiator role exists as a derivation of these two dominant roles. The technical nature of national policy scopes also creates a managerial role, and a conservative coalition imposes a budget-cutting role. Except for a "honeymoon" period following a lopsided electoral victory, the mobilizer role is virtually denied the President because of the existence of powerful coalitions which would be threatened by bringing new participants into the political arena.

The executive's role may be changed or transformed by (1) systemic factors which produce changes in values and power distributions within the system; (2) situational factors which are unpredictable and have consequences for political systems; and (3) the personality of the incumbent. Of the three, the least seems to be known about personality, which may be one of the greatest forces for innovation. Executives then operate at a focal point which forces them to contribute to the stability of the political system but also enables them to influence the rate of and direction of innovation within the system. Actual role transformation, however, is seldom dramatic and typically incremental, because stability throughout the political system is normally functional for most participants.

In attempting to construct a framework within which the role of the executive can be interpreted, the author has been forced to handle a wide variety of social science concepts and deal with virtually the total political system. This is an enormous task, but there seems no realistic alternative if we are to develop a more systematic understanding of the American executive and his role. Professor Ries's work has provided us with an imaginative framework for interpreting existing knowledge about the executive and a basis for additional theory building and empirical research.

LLOYD A. ROWE

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What Price Vigilance? The Burdens of National Defense. By Bruce M. Russett. (New Haven and London: Yale University Press, 1970. Pp. xii, 261. $7.50.)

This admirable work is modern social science at its best. It is concerned with a problem of enormous importance on which may depend the whole future of mankind. This is the problem of what I have called the "war industry," that is, that segment of the activity of society which produces what is purchased with national military expenditures. The book makes no pretense at an exhaustive treatment of the subject; it concentrates heavily on what perhaps is the most significant aspect—what determines the overall size of the military sector? There is only incidental discussion of how, within this overall expenditure, resources are allocated to different forces, different weapons, and so on. The approach is quantitative and statistical, using for the most part, however, the relatively simple techniques of rank-order correlation. It uses these techniques with modesty and restraint, and the author is fully aware of their limitations. One does, however, have a sense of the frontier of knowledge being pushed forward, and even the little advance into the enormous complexity of the social system is very cheering.

The work opens with a general discussion of the problem. Military expenditures in the United States have not only exhibited a "ratchet effect," in the sense that after every war they do not return to their previous relative level, but since the Second World War they have remained at a level and order of magnitude that is ten or twenty times greater, as a proportion of the economy, than what was regarded as "normal" in all previous American history. This is a major quantitative change in American society with enormous consequences not only for American citizens but for the whole world. The author puts forward tentative hypotheses to account for this momentous change.

The second chapter analyzes the roll call votes in the Senate in two years—1961–62 and 1967–68, with a scaling technique designed to create statistically emergent, rather than previously imposed, categories relevant to national defense. Three of these categories emerge for the earlier year and four for the later. In both years, however, the major category is National Defense itself. What comes out is a fairly clear hawk-dove spectrum of both senates, which is however not very closely related to other clusters of voting behavior, such as aerospace, gun control, or even arms sales abroad. A few maps would have enlivened this chapter greatly. In fact, I ventured to draw one myself. It shows up what I have called the "infertile crescent," from the point of view of the doves—the great crescent of hawkishness running through the mountains and Great Plains states to the South, from North Dakota through Wyoming, Colorado, and Texas, to South Carolina.

The third chapter relates defense expenditure by states to the corresponding senatorial voting behavior. The relation is quite low, with the conclusion that on the whole hawkishness is not much related to narrow economic considerations, but rather to general ideological viewpoint. There is no relation at all between hawkishness and defense contracts by states, mainly because most defense contracts go to the dovish states of the Northeast, Middle West, and Far West. There is some relationship, however, with military personnel by states, suggesting that the "permanent armed forces" are more important politically than are fly-by-night defense contracts. There are very interesting suggestions, not followed up much, about tradeoffs between the defense expenditure and civil rights.

The fourth chapter is an attempt to apply the theory of public goods to the problem of alliances. The theory of public goods, as developed by Samuelson, and especially Mancur Olsen and Zeckhauser, suggests that the rich will usually end up paying a larger proportion of their income for public goods than the poor will. In the case of alliances, this principle would be reflected in a positive relationship between the GNP and the proportion of the GNP devoted to defense. For NATO and, in later years, for the Warsaw Pact, this hypothesis works out fairly well, for the Latin American countries not too well, and for the Arab League not at all. These seem like interesting and significant conclusions, which would not emerge without the quantitative and theoretical analysis.

The fifth chapter on "The Opportunity Costs of American Defense" is a careful economic analysis of what goes down when military expenditure goes up, what goes up when military expenditure goes down. The method here is simple correlation using various proportions of the GNP as the variable. The conclusions are interesting; for instance, education emerges as an important victim of defense. The methodology is somewhat open to criticism. Using the GNP as a base neglects unemployment, which could be taken care of if the Gross Capacity Product rather than the Gross National Product were used as the "100 per cent." The influence of the Second World War, also, is so large that it tends to distort the correlations and, though this is recognized sometimes by making the correlations nonlinear, it might have been better to treat the period after the Second World War separately. The next chapter extends this analysis to some other countries, particularly Canada, France, and Britain, with useful comparative results. The work concludes with a brief chapter on the military in American life, which, however, is sketchy and not up to the quality of the rest of the book. There is an appendix by Peter A. Busch on "Mathematical Models of Arms Races" which is interesting in itself, but perhaps would have been better published elsewhere. As this reviewer's work is not even mentioned in it, it is hard for him not to feel that the work is incomplete.

One general criticism might be made of the theoretical framework of the book. It assumes implicitly that the international system is in the phase of "unstable peace," as I have called it (in "Research on Peace," *Science Journal* [England] 5A [October, 1969] 53–59); and there is no suggestion that part of the changes, in the 1960s especially, may be the result of the system's moving in the direction of the phase boundary that leads into stable peace. Consequently, there is no treatment of what might be called the "integrative aspects" of the international system, the slow growth or decline of which has a great deal to do with creating the framework for the system of arms races and deterrence. It can well be argued, however, that this topic is well beyond the scope of the book, and this criticism in no way diminishes the substantial accomplishment of this work within its limited field. One hopes the author will go on to fresh triumphs.

KENNETH E. BOULDING
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The Political Imperative: The Corporate Character of Unions. By Gus Tyler. (New York: The Macmillan Company, 1969. Pp. 294. $7.95.)

The Labor Revolution. By Gus Tyler (New York: The Viking Press, 1967. Pp. 271. $6.50.)

Gus Tyler, a foremost student of American labor, is a highly successful union leader who has served as Assistant President and Education Director of the International Ladies Garment Workers' Union. In both *The Political Imperative* and *The Labor Revolution,* Tyler marshals a lifetime of experience as a participant observer. His wide-ranging concern is indeed refreshing in this era of overspecialization. While both books are written for the general student of organized labor, each contains much of specific interest for political scientists.

In *The Political Imperative* Tyler tells us that he relies not only on his three decades of experience in the labor movement but also draws upon anthropology, ethology, theology, biology, economics, history, psychology, sociology, and cooperative life. He is true to his word; moreover, he also makes use of jurisprudence, semantics, genetics, child behavior, and philosophy in his analysis.

At the outset Tyler posits his main concerns:

> First, will there continue to be a labor movement? . . . what is the role of organized labor in government? Finally, what are the aspirations and ambitions of trade union leaders—democrats or bureaucrats? (p. 1)

The chapter on "Universality of Unions" represents a trenchant historical analysis. Tyler adduces strong evidence to indicate that, ". . . employees, like slaves—are among the last to organize. And among workers, the very poorest and the most illiterate are usually the least likely to organize" (p. 39).

The student of American political parties will find a first-rate analysis of the broker-mediator function of American political parties. Tyler fully understands both the nature of coalition politics and the decentralized operation of American political parties. Furthermore, he provides perceptive insight into the operations of labor politics in New York State. There is a limitation, however. In supporting the theme of nonpartisan politics, Tyler seems to underrate both labor's strong attachments for the Democratic party and its outright hostilities for the Republican party outside of New York state. He overlooks industrial states like Indiana, where legislative votes on the controversial "right-to-work" law always found more than 90 per cent of the Republicans lining up for "right to work," while a like number of Democrats took an opposite stance. Tyler does not demonstrate that his analysis is applicable outside of New York state.

One marvels that a man who can perceptively apply Federalist #10 to his analysis of interest groups, can fail to mention *The American Voter, Voting,* or the other landmark works when analyzing union-member voting behavior. Nonetheless, Tyler offers remarkably accurate conclusions based apparently upon a lifetime in labor politics rather than upon the data reported in the voting studies.

In his final chapter, Tyler addresses himself to a significant problem of democratic theory—striking a balance between the liberties of individual members and the collective requirements of the union. He incisively analyzes the differences and similarities between governmental and various nongovernmental institutions. In addition, he ably discusses the problems of union democracy and calls for a union leadership with the power to lead—that is, to represent the group as well as to protect it. He also attacks those leaders who forget that sovereignty ultimately belongs to the members and argues that too often the leaders equate personal good with the good of the membership. More significant than his actual conclusion is the careful manner in which Tyler examines the case both for and against union democracy.

The book does have a few minor shortcomings—reference to a questionable "herd instinct" and an occasional misplaced analogy (business and labor as siblings engaged in rivalry for the respect of the parent—the social order). Nevertheless, on balance *The Political Imperative* is well worth reading.

The Labor Revolution is a highly optimistic book. Although many are concerned that organized labor can at best engage in a holding action, Tyler forecasts a highly roseate future. The optimism rests upon a number of factors. First, while recognizing that the idealism of the 1930's which held sway in the Northeast and the Midwest is no longer present, he sees the South and the Southwest as the battlegrounds of the 1970's.

Tyler states:

> The South is a twentieth-century fact, resting on a nineteenth-century economy, ruled by an eighteenth-century aristocracy . . . three developments will be of major importance to the labor movement. First, industrialization will provide the traditional base for unionization. Second, the Southern Negro, responding to his new self-image, is becoming active as a unionist and as a voter. Third, these factors, plus the trend in federal law and national opinion, are liberalizing Southern politics (pp. 10–11).

The general triumph of moderate southern candidates in 1970 certainly constitutes a liberalization of Southern politics. Whether this represents a trend or an aberration remains to be seen, since Negroes and labor are still grappling to achieve more than a tenuous coalition.

Another area of Tyler's optimism involves an alliance between young idealists and organized labor. Although the young people who worked for Eugene McCarthy had not fully blossomed when this book was written, the data fail to indicate that they found common cause with labor as did their counterparts of the 1930's. My own surveys of students working for Eugene McCarthy in 1968 actually indicated their strong antipathy to organized labor—more intense even than their antagonism toward American business. The situation still holds. Activist youth apparently will not provide the infusion of idealistic leadership that Tyler seeks.

Tyler also presents a concise but effective history of the labor movement, an astute analysis of featherbedding as a cultural problem not unique to unions, and a discussion of automation and unemployment.

His objectivity is most open to question in the chapter on Negroes and the labor movement. Characterizing George Meany and the old AFL as champions of the Negro seems contrary to the exclusionary policies many building crafts have continuously followed. That labor may have finally come around to verbalizing a pro–civil rights position does not blur the reality of discriminatory practices that still pervade many individual unions.

Tyler believes that the rural legislator is the *bête noire* of progressive policies, and if his overrepresented strength is diminished, liberal legislation will likely take place. As a political realist, Tyler knows that in many instances reapportionment will bring an increase in the strength not of the inner city but of suburbia. He argues, however, that the suburbanite ought to be joined with his urban counterpart since both share the problems of water and air pollution, transportation, housing, and integration. He introduces little evidence, however, to convince us empirically that conservative suburbia will actually start electing activist liberals to the various state houses.

If Tyler can be faulted for being unrealistic on reapportionment, he can be lauded for his understanding of the political system in general. He demonstrates a high degree of political pragmatism in demonstrating the unfeasibility of labor's forming a third party.

The concluding chapter, "Politics in Labor," provides insightful analyses of the conflict between the intellectual and the practitioner. Although Michels originally dealt with this problem, Tyler places it in its appropriate modern American setting. His insights on James Hoffa are also worth reading, as is his plea for more effective education within organized labor. He argues cogently that labor can strengthen its case with the public as well as develop competent leadership by emphasizing meaningful educational programs.

All students of labor politics should read this book for its intellectual breadth, its predominant fairness, and its excellence of style.

MELVIN A. KAHN
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The Impact of the United States Supreme Court: Some Perspectives. By Stephen L. Wasby. (Homewood, Ill.: The Dorsey Press, 1970. Pp. 305. $4.25.)

Since the celebrated case of *Marbury* v. *Madison,* decisions of the U.S. Supreme Court have produced lively discussion, vigorous controversy, and critical examination. Though scholars (and laymen) have engaged in doctrinal analysis and plotted doctrinal trends of court decisions, they have paid little attention to the impact of court decisions on the political process. For the most part, research efforts at systematic examination of the consequences of Supreme Court decisions have occurred only within the last two decades. Many of these efforts were sparked by precedent-shattering decisions of the 1950's and 1960's in such controversial areas as racial discrimination, church-state relations, and criminal law enforcement. These court actions, and the reactions to them, provided the raw material for accelerated interest and study of the Supreme Court from a political perspective. (See, for example, Theodore Becker's *The Impact of Supreme Court Decisions,* Oxford University Press, 1969.)

The volume under review here may well be characterized as a kind of "pause at the way-station" toward the development of "impact" theory. Professor Wasby's purpose is "to pull together and begin to integrate the extant materials on the impact of the United States Supreme Court" (p. vii) and to examine critically what has been done. As a result, he has produced a fairly readable and useful guide to future research in this area.

The volume is divided into two parts—the first focuses on questions essential to the development of a "theory of impact," and the other examines the impact of the Court's decisions in several substantive areas and political arenas. After an initial look at some examples of impact (attempts to overturn, evade, or comply with decisions; consequences for the immediate parties; effect on political and bureaucratic activity, etc.), Wasby strengthens the case for regarding the judicial system and more particularly the Supreme Court from a political perspective. Wasby suggests that such a perspective, as contradistinguished from the traditional constitutional-legal perspective, is essential to theory building in the area of the Court's impact.

Three other important problems examined initially are the conceptualization, the measurement, and the identification of relevant variables and their effect upon impact. Here Wasby considers the "boundaries" of such concepts as impact, compliance, noncompliance, effect, aftermath, and evasion. In doing so, he maintains that some of the difficulties in utilizing or "operationalizing" these concepts have led to a search for other concepts through which more meaningful analysis may result. Furthermore, he notes that measuring and isolating the impact of a decision presents some difficult questions. For example, is one to measure impact only by visible activity? Can impact be isolated as a causal factor? Does one measure merely the number affected by a decision? What time span should be considered?

Among the variables examined are those that relate to "characteristics of the case itself," such as the "closeness of the vote," the existence of a majority opinion or a plurality opinion, the statutory or constitutional grounding of the decision, and the degree of "completeness" or "ambiguity" in the disposition of the questions raised. Others relate to environmental factors, characteristics of particular decisions, followup efforts, community characteristics, and social-psychological factors.

Wasby concludes the first part of his volume with an examination of three frames of reference that might be utilized in the study of impact. They are: 1) the constitutional-legal framework, which emphasizes the courts as legal institutions operating through fixed rules and processes; 2) the political framework, which focuses on the activities of interest groups and emphasizes viewing the Supreme Court as a part of the political process; and 3) the framework of communications processes, which involves looking at the Supreme Court as a part of the communications network in which messages are transmitted to "attentive publics" and are reacted to. Because of their inherent limitations, each of these frames of reference contributes varying perspectives and brings both gains and losses for the researcher.

In the second part of the volume Professor Wasby applies his theoretical perspectives to several substantive issue areas and governmental and political arenas. Considering such issues as legislative reapportionment, obscenity, church-state relations, public school desegregation, criminal procedure, and economic legislation, he clearly illuminates for each issue the Supreme Court action taken and the consequences that have followed. Like many others, however, Wasby is lured into a disproportionate concentration on the "hot" issues of the day —those involving civil liberties.

The political arenas on which he focuses attention include the Congress, the President, the federal bureaucracy, the lower courts, and state officials. Using several Supreme Court decisions, he illustrates the kinds of reactions and responses from these arenas. For example, were there Congressional attempts to amend the Constitution or to strip the Court of its appellate jurisdiction in specific areas, or did a state supreme court refuse to carry out the mandate?

Much of what he writes in Part II is descriptive and somewhat repetitive of materials presented in Part I or in the work of others. Literature chronicling what happened after the Court's decisions in each of the areas considered is plentiful in the work of journalists, lawyers, and social scientists. But what is most valuable in Wasby's work, and what distinguishes it from that of many others, is his emphasis on the impact of a decision on the actors in the various political arenas. Of particular merit is the attention Wasby has given to the lower courts—federal and state. In doing this, Wasby reflects an increasing effort among political scientists to overcome the "upper-court myth." He also suggests the need for greater attention in impact research to the role of state legislative and executive agencies.

The most important contribution of Part II, and perhaps of the entire volume, is the presentation of a series of hypotheses about where we are in the development of an impact theory and how our research must proceed. Wasby, properly cautious, warns that some hypotheses are not yet "tightly operationalized" and "lack precise delineation." But given the gaps in our knowledge of impact, testing these hypotheses empirically would be an excellent starting point.

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Federal Grants-in-Aid: Perspectives and Alternatives. By Deil S. Wright. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1968. Pp. 158. $2.00.)

Much has been written in recent years on the topic of fiscal federalism. Libraries are replete with historical reviews (Elazar, Mosher), normative prognostications (Heller, Reuss), empirical inquiries (Sharkansky, Pogue), as well as some beginning efforts at comparative analysis (Break and May). But what is available for the beginning student of fiscal federalism, or one with a casual or peripheral interest in the subject? Here, with the exception of overview chapters in a few textbooks, one finds a void. Deil S. Wright's little monograph offers considerable relief for this situation. His stated purpose is "to review the several systematic studies that exist in the field, to assess the overall effects of grant programs on what today is called intergovernmental relations, and to offer some thoughts about possible reforms" (p. 1). *Federal Grants-in-Aid* does this and more.

The utility of this work clearly depends on the extent of one's prior knowledge of the grant-in-aid system. The serious student will find little that is original in the chapters which outline the definition, legal precedents, historical development, and causal basis for today's grant-in-aid program. For the novice, these chapters set the debate about future adjustments in the American fiscal system in perspective.

Relying on the 1958 House report on the grant program (by the Fountain Committee), the author gives a careful, comprehensive listing of the philosophical, legal, fiscal, and administrative advantages and disadvantages of the present grant system. One wonders, however, if the Committee Report has not become somewhat dated, given the changes that have occurred since 1957 in the grant-in-aid programs, both in terms of the number of grants and the total amount of money involved in intergovernmental transfers. This period of great growth and change for the program has focused greater academic and public attention on the subject of intergovernmental finances and has also intensified the hopes and fears of many observers far beyond that evident in the testimony of fourteen years ago. Unfortunately, the data are generally interpreted as if very little change had occurred since the 1957 hearings. The research of this reviewer (as well as that of Derthick, Pogue, and Sundquist), suggests that a very different view of the grant-in-aid program would emerge if the Committee hearings were reconvened.

Even with this limitation, Professor Wright's analysis of the grant-in-aid program from the perspectives of the national, state, and local participants is the most useful part of the book. The analysis is based on data collected from a multiplicity of sources: the author's own roll-call analysis of the 86th and 87th Congresses; the 1957 House Subcommittee on Intergovernmental Relations' report, *Replies From State and Local Governments to Questionnaire on Intergovernmental Relations;* the Senate Subcommittee on Intergovernmental Relations' reports, *The Federal System as Seen by State and Local Officials* (1963) and *The Federal System* *as Seen by Federal Aid Officials* (1965); earlier studies by the National Municipal League and the Council of State Governments; and several academic reports. While these accounts present real problems of incomparability of data, low response rates (eight per cent for part of the 1963 survey), and time-boundedness, the reader receives an interesting picture of fiscal federalism in operation. Illustrative is the discussion of the resistance to change by federal grant administrators because of what is termed recurring perceptual themes of "functionalism," "professionalism," "protectionism," and general "indifference"; or the somewhat surprising preoccupation of administrators with program politics; or the amazingly frequent points of communality among state and local officials.

The role of the states is examined with a rather traditional listing of the pluses and minuses that will determine their future. Not surprisingly, the author comes to the increasingly accepted optimistic projection for the middle tier of our federal system: "Our discussion of the debit and credit side of the ledger does suggest that the criticisms of state government have been overstated and the positive features have been understated. In other words, the states are stronger than people think" (p. 127). The author's intimate knowledge of state government notwithstanding, other writers, myself included, would argue that except perhaps for a select few of the states, such optimism is quite unfounded.

The monograph concludes with a summary of the methods of improving the administration of the current grant-in-aid system and suggested alternatives to existing distribution techniques. Included in the latter discussion are direct federal expenditures, federal tax reduction, tax credits, and tax sharing—more specifically, the Heller revenue sharing plan.

A review of the pros and cons of these alternatives leads the author to conclude that "revenue sharing comes off best of all the possible alternatives to grants-in-aid, with the unique tax credit proposal of the Advisory Commission running second" (p. 150). While the last two years have witnessed the emergence of new sophisticated and relevant arguments against revenue sharing, the author does give a reasonably balanced hearing to the debate as it was put forth during the 1964–68 period.

In conclusion, while some of the author's prescriptive and projective observations may be challenged, and, by the author's own admission, numerous data problems are unresolved, *Federal Grants-in-Aid* makes a real contribution for the beginning student of fiscal federalism. It also contains a few meaningful insights for the more mature student as well. It is a work that gives a balance to the literature of fiscal federalism; as such, it is highly recommended.

PARRIS N. GLENDENING
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African Perspectives: Papers in the History, Politics, and Economics of Africa Presented to Thomas Hodgkin. Edited by Christopher Allen and R. W. Johnson. (Cambridge: Cambridge University Press, 1970. Pp. 439. $17.50.)

The seventeen essays in this volume presented to Thomas Hodgkin on his sixtieth birthday are a fitting tribute to his pioneering work in African Studies and his influence on several generations of Africanists. His first book, *Nationalism in Colonial Africa* (1956), quickly became a classic, while his *African Political Parties* (1961) still remains the most useful global analysis of African party structures before the era of one-party states and military regimes. As an historian Hodgkin was also one of the first Europeans to present Africa from an African rather than a colonial perspective.

Generally of higher quality than those found in similar collections, the essays are scholarly, concrete, analytical, readable, and committed—traits which characterize Hodgkin's own work. Despite the variety of topics covered, the volume is cohesive because it reflects much of Hodgkin's main interests and concerns. Part I (Historical Perspectives) opens with four pieces on the Islamic factor in West Africa, followed by five essays on diverse religious responses to the colonial situation. Part II (Political and Economic Perspectives) has eight essays: the first four are on political science and political economy, while the last four are case studies in political change.

The first paper by Hunwick and the second paper by Wilks and Ferguson are best left to historians and Islamists for comment. Although aimed at historians, Jeff Holden's methodological critique of previous evaluations of Samory Touré has useful lessons for other researchers working in Africa. Holden warns against taking at face value colonial records and recommends caution in accepting the results of interviews with people who often tell the stranger-researcher what they think the government would like to hear rather than what they really think.

Three historical pieces with Islamic themes examine responses to colonialism in Guinea, Algeria, and Senegal. Marxist historian Jean Suret-Canale traces Touba's ties with its precolonial, colonial, and postcolonial rulers and notes the constancy of the principles underlying these relationships. His discussion of the P.D.G.'s relations with this influential, religious center indicates that Guinea's ruling party, though pursuing a social revolution in the countryside, continues to respect Islamic traditions. Jamal M. Ahmed's study of the life and work of al-Ibrahimi (1889–1965) stresses the importance of Islam in the evolution of Algerian nationalist thinking. Ahmed's paper is especially interesting because it sheds light on the philosophical premises behind the policy of Arabization now being vigorously pursued by the Boumedienne regime. Donal Cruise O'Brien's short piece attempts to explain the rise of the Mouride Brotherhood in Senegal in terms of the religious *charisma* of its saintly founder, coupled with the more mundane organizational skills of the wordly Ibra Fall, one of Amadou Bamba's early followers.

Essays by Jack Goody and Sholto Cross analyze the emergence of messianic religious leaders in different colonial situations. Cross's biographical essay on Jeremiah Gondwe, a leader of the Watchtower Movement in Zambia, treats his career as a positive and creative response to colonialism. Goody's piece challenges the assumption that African religious revival movements can be explained adequately as results of the dislocations caused by colonialism and signs of resistance to colonial rule.

David Apter opens Part II with a paper in which he turns to Marx as a possible guide in his search for the elaboration of a framework for political science. Although highly suggestive, Apter's essay is too loosely constructed to make a successful synthesis of Marx's work and his own approach to political science. In contrast to Apter's speculative piece are Gavin Williams's and Ken Post's attempts to apply Marxian analysis to concrete cases. After presenting an interesting typology of an ideal-type neocolonial economy, Williams discusses social stratification in Western Nigeria using Weberian as well as Marxian concepts of class and social relations. Post uses Engel's conceptualization of ideology as false consciousness with great effectiveness to analyze the Jamaican Ethiopianism movement, which reached its peak in the 1930's.

The essays by Reginald Green and Ann Seidman are useful contributions to the study of African political economy. After describing the operations of foreign firms in Africa, Seidman offers several suggestions for maintaining national control over them. Green's long essay is a veritable primer on the political economy of decolonization and a marked improvement over *Unity and Poverty: The Economics of Pan-Africanism* which he coauthored with Seidman. Identifying concentrated external dependence as the essence of neocolonialism, Green then examines several African economic strategies and puts forward Tanzania as a possible model for decolonization.

The four case studies concluding the collection support the editors' claim that microanalyses are more likely to facilitate understanding of the main political events and trends in Africa than those built around broad and often fuzzy concepts like "political development" or "national integration." R. W. Johnson's analysis of the successful suppression of the left-wing faction of the P.D.G. in Mamou tells us more about Guinean politics than scores of articles on the political doctrine of Sékou Touré. Basil Davidson traces the causes underlying the rise of revolutionary movements in Portuguese Africa in his contribution to the volume. Christopher Allen's detailed study of Gambian trade unionism provides a greater understanding of the fragility and weakness of African trade unions, while Martin Staniland, in his piece on nationalism and communal partisanship in Bongouanou, Ivory Coast, presents a clear picture of the political processes operating between center and periphery during the late colonial period.

Besides the high quality of the essays, what makes *African Perspectives* a stimulating and provocative volume is the authors' conscious efforts to challenge the conventional wisdom on their topics (even that established by Hodgkin himself on occasion) and to offer alternative methodologies and explanations. These features combine to make the collection a fine tribute to their mentor and a significant contribution to African Studies.

SHELDON GELLAR

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The Nordic Council: A Study of Scandinavian Regionalism. By Stanley V. Anderson (Seattle: University of Washington Press, 1967. Pp. 194. $7.50.)

This is not a book about integration, but on the contrary a case study of nonintegration. The picture that emerges from its pages is one of a Scandinavian regionalism unable to make a place for itself, caught between narrower nationalisms and wider European regional movements.

If there ever were likely candidates for regional unification, a fair case can be made for the Nordic countries. They are, with a few exceptions, closely connected in an impressive number of ways—geographic proximity, language, race, religion, trade, history, and compatible political systems. They have had a record of successful functional cooperation, e.g., Scandinavian Airlines System, common negotiators in the Kennedy Round, a common labor market, and reciprocal social security systems. There are frequent contacts among fraternal political parties, civil servants, and cabinet ministers. In political style and culture they are considerably less alien to each other than, for instance, Mississippi is to Maine.

And yet, as Professor Anderson makes clear, these cultural and historical affinities are neither necessary nor sufficient conditions for integration. In the absence of a volitional unifying act, the consciousness of distinguishing characteristics has invariably turned out to be greater than the sum of shared values, and the economic advantages of EFTA or EEC have overridden the periodic attempts to create a Scandinavian common market.

Despite a growing literature on regionalism, the term remains unamenable to satisfactory definition. This difficulty may arise because there are many different kinds of regionalism, depending on the region in question or on the purpose of any particular regional arrangement. The definition which underlies this book is, in light of the subject matter, suitably unpretentious: "officially sanctioned continuous effort to increase regional harmony by peaceful means. Similarities are heightened and differences lessened" (pp. 8–9). This definition is clearly intended to be broader than those meanings that emphasize regional integration or defense, and in this respect the author's caution is not misplaced. With Denmark, Iceland, and Norway in NATO, Sweden is neutral by choice and Finland by necessity. Earlier attempts to form a common Nordic defense pact could not overcome these fundamental differences, and attitudes towards the desirability of EEC membership are widely divergent. Under these circumstances, the Scandinavian countries have not moved perceptibly in the direction of supranational organizations, but have instead chosen the path of cooperation ("peaceful change by persistent joint efforts to increase mutual advantage" p. 147.)

The chief organizational expression of Scandinavian regionalism has been the Nordic Council, and the bulk of the book is devoted to an extended description and analysis of the Council. In successive chapters, the author discusses the Scandinavian region; interparliamentary predecessors of the Nordic Council; its organs, procedure, and relations with Scandinavian governments; common Nordic market negotiations; and concludes with some generalizations on the precise nature of Scandinavian regionalism and what we may extract from that experience. The appendices contain the Statute, Rules of Procedure, and Directions for Committee work of the Nordic Council (all annotated) along with the text of the Helsinki agreement and the Statute of the Nordic Inter-Parliamentary Union.

When this book was written, the Nordic Council had been in operation for about fourteen years, and its record in practice must have proved disappointing to any who had hoped that it might provide the nucleus for a United States of Scandinavia. Instead, it was to serve as little more than a sounding board of Parliamentary and public opinion on issues of inter-Scandinavian relations, an intergovernmental substitute for authoritative regional decision making. One sign of its weakness was the dominance of its Presidium and Secretariats, despite the prominence and continuity of service of the elected members. The tasks performed by the Nordic Council have been of a kind more appropriately performed by civil servants (preponderantly Swedish and Danish) than by politicians. The absence of dissent, arid discussions, excessive compromise, and reduction to levels "desired by the least-regionally minded member" created the impression that the Council was, to paraphrase one delegate, in danger of drowning in its own unanimity (p. 77). The Nordic Council in effect became a kind of pressure group—but one attended with a notable lack of success in penetrating either governments or publics. There is little doubt that the governments of the region are the real decision makers, and that a great many changes would have to be effected before the Nordic Council could acquire the authority it now lacks. (See especially pp. 106–108, which emphasize its present nonauthoritative status.)

The book is detailed, informative, and helpful to any serious understanding of the Nordic Council and inter-Scandinavian cooperation. It is commendably restrained on a topic susceptible to the distortion of integrationist sentiment. It is likely to disappoint those who are looking for prodigious leaps into deep theoretical waters, but this was not the intention of the author, and quite rightly in view of the realities of Nordic regionalism. The author is, however, somewhat more cautious in generalizing than he need have been, and one finds oneself wishing for more extended comment in those passages devoted to appraisal and generalization.

Some of the data could have been brought up past 1962 (e.g., Chapter IV). But, although there have been developments in Scandinavian regionalism since 1966, (e.g., the NORDEK negotiations and revival of the EEC question) none of these really impinges on the usefulness of the book. In a larger sense, it can serve as a reminder of the homely truth that not all regionalism need involve integration in the currently acceptable senses. Full-blown unification does not seem to have been entertained seriously as an objective, and its lack of attainment may not really amount to failure.

JOSEPH B. BOARD

Union College

Priming the German Economy. By John H. Backer. (Durham, N.C.: Duke University Press, 1971. Pp. 212. $6.75.)

John Backer was a member of the American Military Government in the economic administration of the American zone of Germany. Among his assignments was the promotion of exports from Bavaria; as he proves in his book, he had an excellent vantage point from which to observe the achievements and failures of American economic policy. Subsequently, he served as a diplomatic officer in the Soviet Union but apparently never lost interest in his earlier experiences in military government; shortly after retirement from the foreign service he wrote *Priming the German Economy*. It should be emphasized that the book is by no means autobiographical—the reader will find the author in first person in only very few passages of the story—but there is little doubt that the author has fully used his valuable experience in the selection of materials and in their mature and objective treatment.

The book begins with a very clear account of the drafting of United States occupational policies in the middle of World War II. Roosevelt, Morgenthau, Hull, and Stimson all had a hand in fashioning these policies, and Backer shows how they in turn were influenced by military men and civil servants both in Washington and abroad. The result of their labors, the compromise codification in the Joint Chiefs of Staff Directive 1067 determined the overall Allied economic policy in vanquished Western Germany at least until July, 1947. The author is able to show what the administrators of this policy in the field were able to do under the clauses "to protect the safety and meet the needs of the occupying forces" and "to prevent starvation or such disease and unrest as would endanger these forces." No other rationale of dealing with the German economy was permitted under this directive, and one shudders to think what would have happened if these escape clauses had not been used fairly liberally.

With equal skill the author explains how the "thorny problem of reparations" was handled after World War II, quite differently from the post–World War I period. He carefully shows how the United States reparation position was arrived at and how it was changed in negotiations with the Russians. He also traces the relationship between the Allied reparation plans and industrial development in the western zones of Germany. There is a shorter but equally good account of the consequences of the monetary collapse on the German economy and the steps taken by the bizonal administration, and later the Germans, to deal with them. Finally, and here the author seems to have the longest experience, he tells of the efforts of the Joint Export-Import Agency to promote exports from the American and British zones, so that proceeds could be obtained which were used to pay for vitally needed imports. Anyone who has observed recent West German successes in the commercial field—less than 25 years later—and has seen German foreign exchange reserves exceed those of any other country, including the United States, can hardly believe the efforts that had to be undertaken by relatively few military and civilian administrators, both Allied and German, to achieve exports of just barely more than $200 million for all of 1947! The story ends with the onset of the European Recovery Program and with the rise of West German agencies and personalities that take over the functions of military government. The author argues convincingly that at least twenty-four months could have been gained for the German economy if there had been a currency reform in the Summer of 1946 rather than in 1948 and if General Draper's proposal to make 31 billion of industrial raw materials available could have been implemented. *Priming the German Economy* makes it amply plain why, mainly for domestic political reasons in the United States, these steps were not taken earlier.

To this reviewer there are three weaknesses in this otherwise excellent book. First, is it really necessary to reproduce so many documents relating to the occupational policies in full? In some instances exact quotation is essential; in others salient points and summary would do and render reading easier. Second, the author is apparently not as much at ease with economic theory as he is with political reality. For instance, the Keynes-Ohlin controversy over reparations in the interwar period is condensed into one paragraph which, to put it kindly, should have been omitted. Lastly, although the author is extremely fair and generally tolerant of criticism of his own positions, he makes an exception in the fledgling opposition to American economic policies by German politicians in 1947. While few would rise to defend the Halmssen Report, Backer's criticism of Dr. Johannes Semler is not sufficiently supported by the author's argument. Maybe Semler was excessively political in his criticism of American occupational policies and in his questioning of United States motives; but was this not the kind of stuff necessary for the establishment of a new political system and an economic democracy in West Germany?

F. A. Breier

University of San Francisco

Agricultural Production in Communist China 1949–1965. By Kang Chao. (Madison: The University of Wisconsin Press, 1970. Pp. 357. $15.00.)

Specialists interested in policy making in socialist countries governed by Communist parties will find this study informative, stimulating, and useful for general comparative purposes. Since few economic studies of socialist systems relate economic performance to organizational change and policy making, this work is of special importance as an exercise in the political economy of socialist countries.

Professor Chao leads off with an excellent summary of controversy within the Chinese Communist Party about how to collectivize pygmy farms using primitive technology. The leadership, he tells us, agreed upon the necessity of violent land reform but split upon the issues of when and how to collectivize family farms. The technocrat faction, represented by Liu Shao-ch'i and Teng Tzu-hui, argued for a longer transition period under private ownership of land before collectivization, at least until China could begin to produce enough farm machinery. The Maoist faction urged that collectivization immediately follow land reform, arguing that the longer the 'rich farmer' mentality survived amongst the peasantry, the more difficult it would be to socialize agriculture later on. Debate between these factions continued and tensions mounted until the Cultural Revolution resolved the conflict in favor of Mao's leadership and policies. The progress of collectivization through the 1950's and early 1960's, traced against this background of agrarian policy dispute with great skill by the author, makes for a fascinating account of how party discord influenced overall agrarian policy.

Part Two, consisting of five chapters, provides the best account so far in English of Chinese Communist efforts to supply agriculture with new technology and capital. The author and his associates have culled from an impressively large collection of periodicals and technical studies a wealth of information and data on progress of mechanizing farm production, developing irrigation facilities, introducing new cropping systems and seeds, and applying new chemical fertilizer. Have administrative efforts under collectivization produced a technological revolution? Not yet. While some gains were achieved by irrigating more land, cropping land more intensively, and developing some new, higher yield seed varieties, basic errors in introducing new technology to agriculture have caused irreparable harm to farmers and production: the wrong crop systems have been introduced; new plows have been misused; numerous water conservation schemes have caused a greater incidence of floods rather than providing farmers with a stable supply of water.

These examples strongly indicate that the party leadership prematurely emphasized mechanization (p. 116) and hastily introduced new seeds without testing against local conditions (p. 172). The leadership still has not recognized that a first step transformation of agriculture requires a biological revolution based upon the diffusion of new, high-yield seed varieties suitable for local conditions. This policy is difficult to carry out because administrators must be patient and await the results of the technicians and scientists involved in such projects. The party leadership has clearly demonstrated that other priorities come first, and that seed research is too slow and time consuming a strategy for raising farm production. As long as the leadership persists in this belief, it is very unlikely that sustained and rapidly rising yields for foodgrains and industrial crops can occur.

The final section considers agricultural production performance under collectivization. Professor Chao and his associates estimated provincial sown area for foodgrains from data collected from newspapers and periodicals. Their estimate of foodgrain sown area for 1949 approximates that computed by John L. Buck, which is considerably higher than the Communist estimate (p. 207). Chao next adjusted provincial crop yield data in order to multiply by sown area to obtain foodgrain supply estimates. It is not clear how he adjusted certain official yield figures and whether or not he used a weighted average of provincial foodgrain yield to multiply by sown area. The correct procedure would have been to determine the percentage of sown area by crop, a very difficult task, and to multiply each amount by the respective crop yield. His final statistical results show that between 1952 and 1957, foodgrain production increased at an annual growth rate of 2.3 per cent, roughly equivalent to population growth. Chao's estimate stands below official claims but above the estimates of western economists such as Liu and Dawson.

Chao also attempts to determine the productivity of the combined contribution of labor, draft animal, and tractor power, irrigation, and fertilizer inputs to foodgrain production. As this exercise applies to only six years, involves very imprecise measurement of each input's change, and assumes a very questionable weighting procedure of each input's contributing share, his findings call for considerable caution and skepticism. His derived index input of productivity shows a decline of 1.7 per cent per year between 1952 and 1957. Such a finding implies that the growth rate of each input's supply had to increase rather rapidly to enable foodgrain production to increase at 2.3 per cent per annum. Chao fails to carry through in this exercise to show the required increase of each input to produce this growth rate while input productivity declined. One final critical comment: the author's statistical estimates would appear more convincing and valid had he presented them as statistical ranges with error estimates. If economists continue to estimate production performance in Communist China, as they will do, they should present their results as statistical ranges with some error estimate to inform the reader how wide of the mark the estimates conceivably are. Their findings will then carry greater credibility and elicit more respect.

To sum up, this study combines a careful and clear discussion of statistical inference and estimation with policy controversy and organizational change to give the best descriptive and analytic account of agricultural change in Communist China yet written. Chao's findings that foodgrain and technical crop production barely matched population change certainly appears plausible and valid. This reviewer regards such a performance as rather remarkable given the drastic organizational changes and upheaval which have affected traditional family farming. This performance, however, probably attests more to the experience and capability of the Chinese farmer than to the new organizations within which the farmer now must work and live. This fine study makes it abundantly clear that unless the policy of introducing new technology to farming is changed, Chinese agriculture cannot experience the 'green revolution' now taking place in various parts of Asia and long experienced by Japan and Taiwan. This work should find its way onto the shelf of all specialists interested in economic development and political change in socialist systems.

RAMON H. MYERS

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Economic Development in the Soviet Union. By Stanley M. Cohn. (Lexington: D. C. Heath & Co., 1970. Pp. 135. $10.00.)

This is a monograph with many virtues, but here and there it loses its way. The author is at his best when he is handling figures and explaining their meaning. The chapter entitled "Analysis of the Soviet growth model" is full of useful comparative data, and the author seeks in a very interesting way to identify the factors which contributed to Soviet growth performance, among them: natural resources, education, migration from village to town, investment priorities. Perhaps there is one presentational problem of a statistical kind, which deserves a few pages of explanation. Soviet growth indices, especially in the period 1928–37, are highly sensitive to price weights. Thus using Bergson's recomputations, Soviet labor productivity during these years increased either by 1.8 per cent or by 8.0 per cent per year, depending on the weights used. The first figure is poor by international standards, the second is highly creditable. The reasons for the disparity lie, of course, not in any deficiency in Bergson's statistics which Cohn uses, but in the drastic structural and price changes of those years. Surely the reader needs to be told this in detail. And is it not a little misleading to give, in historical comparisons, a single figure for all countries except Russia, as if they were not sensitive to price weights too? Alexander Gerschenkron is quoted several times in his capacity as economic historian, and quite rightly so, but Gerschenkron was also the author of an admirable study which pointed to the extraordinarily high price sensitivity of the index of machinery output in the United States in the period 1899–1939. Cohn might well reply that detailed technical examination of index numbers is beyond the scope of this short work, and it is. But one would wish for a brief discussion of such matters all the same, because they are germane to the proper understanding of the figures.

A rather different criticism concerns the presentation of sources. Where Soviet figures or data exist, it seems only right to quote the Soviet original, or explain why—as in the case of indices of GNP—it is necessary to use the best available western recomputation. But if an actual Soviet-published figure is used, as for instance the number of men in the Soviet armed forces in 1950, then it is slightly odd to find Bergson cited as a source. After all, Bergson presumably reproduced the figure cited in a Krushchev speech; he did not compute it. It seems even more odd of Cohn to cite a paper of Michael Kaser's on economics of education as a source for the rate of growth of Japanese GNP. Surely Cohn would not allow his own graduate students to do this! Needless to say, the reviewer wishes to imply no disrespect to either Bergson or Kaser; he would be just as critical of anyone who cited one of his own works as a source for the date of the death of Stalin, say.

There are some slips in the historical section. Thus it is not the case that "the Fifth Five-year plan (1950–55) was never published." It was presented to the 19th party congress by Malenkov in Stalin's presence. Nor did Trotsky expect the World's workers to be impressed by a "model of a genuine and successful socialist state" in the USSR, since he denied the possibility of socialism in one country.

Yet the book contains many valuable observations and statistical analysis of a high order. Its defects seem to me due primarily to uncertainty in the author's own mind as to its purpose and its readership.

A. Nove

University of Glasgow

West African Chiefs: Their Changing Status Under Colonial Rule and Independence. By Michael Crowder and Obaro Ikime. (New York: Africana Publishing Corporation and the University of Ife Press. Pp. 453. $18.00.)

Historians of colonial and postcolonial Africa might do well to recall the "debate" between Alexis de Tocqueville and Edmund Burke over the French Revolution; for in Tocqueville's analysis of the *ancien régime* and in Burke's *Reflections* one may find accounted for, in terms of national political experience, those divergent ideas and practices that characterized the two nations' colonial empires in Africa and that still deeply influence the political life of "anglophone" and "francophone" regions of the continent. Burke's implication that France in 1789 might well have had a British-type 1688 revolution instead is decisively answered by Tocqueville's demonstration that on the eve of the revolution, France had long since been irrevocably committed to bureaucratic centralism. Britain, meanwhile, remained attached to those "customary" institutions that were to develop into what is called, with a particularly British resonance, "local government."

Thus it happened that, while local circumstances sometimes led to similar results on both sides of an Anglo-French border, the men who administered British colonial Africa generally cherished gentry ideals (even when not of gentry origin), while their French counterparts, as Pierre Alexandre notes in his contribution to this volume ("Chieftaincy in French West Africa") were usually republican, which is to say universalist, egalitarian, and anticlerical in sentiment. Their attitudes toward chieftaincy were correspondingly different (though again local circumstances might produce prudential convergences). The French chief was generally an appointed *fonctionnaire,* his British counterpart more often a local ruler with some claim to traditional legitimacy. British administration concerned itself far more with local government (which incorporated chiefs), with indigenous courts and law, and with the mastery of local languages, while the French emphasized administrative efficiency and the diffusion of the French language. There is currently in vogue a revisionist view that imperialism in Africa was essentially the same everywhere, but while it is useful to point out similarities where they existed, the revisionist argument has, I think, been overdone, as the studies collected in this volume show. The editors seem, in their introduction, to agree, and they point to the consequent differences in the fate of chiefs under independence in French and British areas. Britain and France *were* different in the nineteenth century, and their colonial policies and practices reflected the difference. As Winston Churchill remarked in another connection, "it will take many ingenious books to raise this issue to the dignity of a historical controversy."

This volume presents papers delivered at a symposium held in 1968 at the Institute of African Studies of the University of Ife in Nigeria, and it is to be welcomed as the first major publication of that University's Press. Its strength is the series of detailed accounts of chieftaincy under colonial and postcolonial conditions in various parts of formerly French and British West Africa. Jean Suret-Canale provides an excellent discussion of Fouta-Djalon, and J. Lombard gives a fine treatment of the Bariba of Dahomey. I. A. Asiwaju compares two Yoruba states: Meko and Ketu, one under French, the other under British rule—incidentally bringing out very nicely the Anglo-French difference. Elliott Skinner deals very skillfully with the Mossi, whose ruler, the Moho Naba, was the most durable of African monarchs to fall under French rule, and the one to whom the French perhaps conceded most during the colonial period. He was also shrewd enough to steer through the intricacies of immediate pre- and postindependence politics and emerge with what Skinner feels is a reasonable chance for survival.

Discussions of formerly British territories include John Smith's on the relationship between British officers and chiefs in Northern Nigeria, John Paden's on Kano, Ronald Cohen's on Bornu, J. A. Atanda's on Oyo, E. A. Ayandele's on Ijebu, and S. A. Akintoye's on the Ekiti. All these are in Northern and Western Nigeria, where traditional rulerships were strong. Something like the same situation prevailed in Benin, described by Philip A. Igbafe, and among the Itsekiri, about which Obaro Ikime writes. In Eastern Nigeria, on the other hand, the policy of "indirect rule," applied by Lord Lugard in the North, and later extended to the West, ran into difficulties, for the Ibo and other peoples of that region did not possess strong rulers of the sort found in the North and West. G. I. Jones's account of the effort to create chiefs there does much to clear up earlier confusions on the subject.

The volume includes several discussions of chieftaincy in Ghana and one in Sierra Leone: in Northern Ghana by Phyllis Ferguson and Ivor Wilks, in the "foreign quarter" (*Zongo*) of Kumasi by Enid Schildkrout, in Larteh by David Brokenshaw, and among the Limba by Ruth Finnegan and David J. Murray. Perhaps the best of these is Brokenshaw's analysis of the "resilient" chieftaincy in Larteh.

I would have appreciated more attention than the volume gives to indigenous law and courts, a subject which I think might have thrown further light on the Anglo-French difference. British officials were very concerned with these matters, while French ones *appear* to have ignored or suppressed them. Furthermore, the discussions in the seminar (assuming that the published volume reflects them accurately) might have benefited from a somewhat less exclusively *West* African focus. The notion that "indirect rule" through indigenous political institutions originated with Lugard's administration in Northern Nigeria would not have occurred to anyone familiar with *East* Africa, or, indeed, to anyone attentive to Lugard's career. Margery Perham, in her biography of Lugard, makes it clear that his prior experience in Uganda was crucial to his formulation of the idea (if not the term), and he had still earlier served in India, where the princely states provided a model. Actually, the idea is probably as old as imperialism itself. I also miss any reference to the very considerable literature on central and southern Africa that addresses the very questions that preoccupy the authors of this volume. One appreciates the authors' concern to develop the Anglo-French comparison within West Africa, but it would be a pity if African Studies were to break up into self-contained regional blocs.

Lloyd A. Fallers

University of Chicago

The Diary of a Diplomat in Russia, 1917–1918. By Louis de Robien. (New York: Praeger, 1970. Pp. 318. $8.50).

Civil War in South Russia, 1918. By Peter Kenez. (Berkeley: University of California, 1971. Pp. 351. $10.00.)

The Victors' Dilemma: Allied Intervention in the Russian Civil War, 1917–1920. By John Silverlight. (New York: Weybright and Talley, 1970. Pp. 392. $10.00.)

The Russian Revolution occupies a central place in the history of the twentieth century, though not so important as the one in which Soviet leaders and scholars have enshrined it. Similarly, the role various Western states and Japan played in the Russian Civil War has also been important, although not so great as that assigned to it by Soviet politics and Soviet mythology. Both the revolution and allied intervention have attracted enormous attention, not only from scholars and politicians but also from novelists and film-makers. During the past twenty-five years, as Soviet-American relations have emerged as a crucial factor in international politics, these historical developments have created almost an industry among scholars and would-be scholars.

These volumes represent three different kinds of contributions to this search for knowledge and understanding. Count Louis de Robien was a young Breton lawyer who learned Russian at the School of Oriental Languages in Paris and who was assigned to a junior post in St. Petersburg early in 1914, when he was only 26. He began his diary in March, 1917, with the coming of the first revolution of that year, and he continued it through 1918, with one long interruption. The diary was published originally in French in 1967, almost a decade after de Robien's death, which followed a lifetime of service in the Quai d'Orsay, climaxed by appointments as Ambassador to Romania and as Chief of Personnel at the Quai d'Orsay. Unfortunately, the English translation provides no introduction or footnotes to aid the reader, and the volume lacks a map of Petersburg-Petrograd, which would have been most useful for both the scholar and the ordinary reader. Moreover, *The Diary of a Diplomat* has serious shortcomings as a source and as a guide to the activities, mood, and temper of 1917–1918. De Robien was a junior officer and one with profound aristocratic views, which made understanding the Russian scene very difficult for him. He was not a skilled observer, and the information he provides is often fuzzy or unclear. However, his diary does provide significant insights concerning the allied diplomatic community and concerning life in Russia's capital during a time of incredible confusion and agony. He saw quite clearly that the Bolsheviks were likely to triumph because they alone pledged immediate peace and because the Russian will to fight was absolutely dead. His descriptions of demonstrations, of street fighting, of the effects of the transport crisis (although one could get Black Sea oysters in some restaurants in the summer of 1917), and of the qualities and views of the aristocrats whom he knew are all valuable. Above all, de Robien on occasion showed remarkable insights concerning probable developments, handicapped as he was by his political views.

Professor Peter Kenez, of the University of California at Santa Cruz, is a young foreign-born, American-educated scholar whose volume undoubtedly represents the revision of a good Ph.D. thesis. His book is therefore a case study, a monograph on the beginnings of the Russian Civil War in the South. It is especially valuable for its explanation of the failures of the Volunteer Army and of the Whites in general: their glaring disunity; the absence of effective leadership; the conflict between their dreams of "a Russia Great, United, and Indivisible" and the goals of the national minorities; the organizational abilities of the Bolsheviks; and the lack of political sense shown by the Volunteer Army generals. Indeed, the Kenez study illuminates the need for a study of the military leadership of the Czarist forces. What kind of professional and general education had it received? Why were the generals so utterly lacking in political sense?

Kenez provides no bibliographical essay or annotated bibliography, and the bibliography has curious omissions. The extensive use he made of the Hoover Library and of the Russian and East European Archives at Columbia University demonstrates how valuable these resources are and what a significant contribution its collectors have made. He makes no reference to Soviet sources, although he has studied in the Soviet Union.

In summary, *Civil War in South Russia, 1918* is an interesting and valuable book, a first volume of which a young scholar can be proud. At the same time, perhaps because the subject itself is so vast and involves so many other contemporary developments, the volume contains several serious flaws. It devotes insufficient attention to the fighting itself and to factors such as supplies. It is not clear about the significant role Germany's changing position played. Above all, it tries to isolate the Civil War in the South from the rest of the canvas, including the climactic year on the Western Front and the policies of Russia's allies.

Silverlight's *The Victors' Dilemma* satisfactorily discusses the incredible problems these allies faced in trying to comprehend and deal with the Russian revolutions, the Bolsheviks (whom we understand with difficulty even now), the various national minorities seeking to obtain their independence, the various other anti-Bolshevik groups, and the general confusion within Russia; it also deals well with the crises in the West caused by the Germans' final unsuccessful thrust for victory in 1918, their sudden collapse, the end of the war, the flood of problems revealed by peace, and the political pressures within and among each of the victorious powers. His fair-minded, lucid description is a very considerable achievement and no doubt represents in part the skills Silverlight has acquired as assistant editor of the British *Observer*. Above all, he succeeds in enabling the reader to understand the extraordinary complexities of the issues with which the allied leaders had to deal, at a time when the entire world was in disarray. The information they could obtain on developments within Russia was scanty, tardy, and unreliable. Often, intelligent decisions, based upon a sound understanding of the national interests of the states involved, were upset or reversed by quirks or flukes. Silverlight argues, with some justification, that the decisive factor was the action in the summer of 1918, of the Czech troops, eager to leave Russia to fight for the creation of an independent Czechoslovakia and convinced that the Bolsheviks were seeking to block them. One of the many ironies in this genuine tragedy of errors was that the war ended and Czechoslovakia became a free state before the Czech troops could have arrived, even if the Bolsheviks had sought to assist them.

This volume relies heavily on the most recent and most able academic studies of this tangled period, upon the memoirs of British statesmen in particular, and especially, upon British Cabinet Papers, which were opened to the public in 1966. It uses no Soviet sources, undoubtedly because Silverlight does not read Russian. Its maps are unclear and not especially useful. It was also written for educated Englishmen who possess more knowledge and understanding of British politics than do Americans and other foreigners. But it is an excellent general volume which should increase our understanding of the problems statesmen faced fifty years ago and face still today.

ROBERT F. BYRNES

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Economic Reforms in Eastern Europe. By Michael Gamarnikow. (Detroit: Wayne State University Press, 1968. Pp. 202. $8.95.)

In all political systems there is a definite relationship between economic and political developments, but nowhere is the relationship as close as in communist states. Since the days of Stalin, almost all communist leaders (Marshall Tito of Yugoslavia is the exception) have maintained a tight grip on the economies of their countries through strict control over the central planning apparatus. In this way the communist leaders have determined investment priorities, resource allocation, and the proportional development of the economy—invariably favoring heavy industry and defense over agriculture and consumer goods. By the mid-'sixties this system of "command economy" had achieved some notable successes in the development of heavy industry. But by that time the communist leaders of the Soviet Union and Eastern Europe also realized that their economies were in serious trouble. The capital-output ratio (the amount of capital needed to produce a given unit of output) was increasing at an alarming rate, labor productivity remained distressingly low, and piles of shoddy and unsalable consumer goods had begun to accumulate in state warehouses. The situation was particularly acute in the nations of Eastern Europe, which lack the abundance of raw materials that has enabled the Soviet Union to "muddle through" its economic difficulties. It was as a result of these economic pressures that the communist leaders of Eastern Europe, in many cases reluctantly, began to experiment with reforms of their command economies, and it is this process of experimentation in the years 1964–1967 which Michael Gamarnikow describes in *Economic Reforms in Eastern Europe.*

Of greatest interest to political scientists in this very useful book is Gamarnikow's description of the political obstacles encountered by the advocates of economic reform, who were primarily economists. Although the pattern of economic reform differed from country to country (Gamarnikow provides an excellent comparative treatment of the reforms), almost all reformers agreed on the following three points: decentralization of the economy by giving greater autonomy in decision making to individual factories; the establishment of profit rather than gross output as the criterion for judging the effectiveness of a factory; and the introduction of a more rational price system related to scarcity costs and based on the needs of the market, in place of the existing administratively determined system of prices.

There was a great deal of political opposition to reforms of this type. In the first place, the central planning apparatus was very reluctant to reduce its power by granting the individual factories more autonomy. Secondly, many party leaders were fearful of losing control over the economy if too much initiative were left in the hands of the factory managers. In addition, since the allocation of scarce economic resources is one of the party's major powers, many communists were reluctant to give up the "communist pork-barrel," in which they were able to construct huge enterprises in politically desirable areas—irrespective of the economic rationality of such investment decisions. Another source of opposition came from those individuals, often with good party connections, who stood to lose their jobs as a result of the reforms. These included not only members of the central planning apparatus, but also factory managers who owed their jobs more to political loyalty than to economic ability. Indeed, such individuals, good at carrying out routine orders from above in the system of command economy, proved unwilling to take on the additional responsibilities and challenges which would be necessitated by the reforms.

Underlying the whole debate about economic reform in Eastern Europe has been the question of whether genuine economic reform, which would mean the establishment of a pluralistic system of autonomous economic enterprises guided by signals from the market rather than orders from the party-controlled central planning apparatus, would eventually lead to the democratization of the East European communist states. According to Marxist theory, the economic "base" determines the political "superstructure," and, while the reverse has hitherto been the case in communist practice, many of the economic reformers, and Gamarnikow himself, seem to feel that such a process of democratization is inevitable. While I am not quite so sanguine, it is clear that many of the communist leaders of Eastern Europe have been preoccupied with just such a concern. The end result has been that the Eastern European communist leaders, with the exception of Janôs Kadar of Hungary (and of course Tito) have been endeavoring to allow just enough economic reform to make their existing systems of command economy work more efficiently, but not so much as to jeopardize their monopoly of political power. This economic "juggling act" is a very difficult one, and, as the fall of Gomulka demonstrated, the success of such halfway measures is by no means assured.

In presenting these issues in a comparative way, and in a manner easily intelligible to political scientists, Mr. Gamarnikow has done a service to students of Eastern European affairs. It is hoped that he will soon update his book by adding a discussion of the effect of the Soviet invasion of Czechoslovakia on the process of economic reform in Eastern Europe and in the Soviet Union itself.

ROBERT OWEN FREEDMAN
Marquette University

Colonialism in Africa 1870–1960. Volume Two: The History and Politics of Colonialism 1914–1960. Edited by L. H. Gann and Peter Duignan. (Cambridge, England: Cambridge University Press, 1970. Pp. 563. $17.50.)

This is one of those reviews that tell you what the book is not, as well as occasionally discussing what it is. Works that attempt grand summations of forces and events are fair game for such an approach. The present work is nothing less than "the second volume in a five-volume collaborative history of the colonial era in Africa" (front flap blurb). The editors define their project in the first two volumes as "essentially a work of history . . . designed to interpret the underlying motivation of what has often been called 'the New Imperialism'" (p. ix). Succeeding volumes will deal with the social impact of imperialism on the peoples of Africa, with economic forces, and with bibliographic and archival sources.

Other academicians can decide to what extent the aspirations of their respective fraternities are fulfilled. Despite equal billing for the politics of colonialism, political scientists will be disappointed. "Who gets what, when, and how" seems largely a public and official affair. Two chapters out of fourteen concentrate on black African responses to colonial rule. Immanuel Wallerstein notes important but not basic social changes at the macro level, while Martin Kilson presents evidence—largely from Ghana—of the rise of new skill groups at the micro level. There is no chapter on trade unions, rural associations, or political parties, despite the fact that political scientists have devoted just about all their research efforts in Africa on these matters in the past fifteen years. Crawford Young's chapter on decolonization emphasizes Metropolitan politics, a vantage point that leads to overestimating the possibilities for controlling events once political challenges get under way. The historian A. A. Boahen, in the chapter entitled "Conclusion," notes the glaring omission of any treatment of African parties between 1944 and 1960 (p. 517). Is it significant that Boahen is the only black African contributor? Since the editors saw fit to include an "Epilogue," which in part disputes Boahen's view, the absence of more extended discussion in the body of the book is even more inexplicable.

Continuing in this vein, what else "ought" political scientists to expect? For one thing, a new look at the connections between foreign policy and domestic politics—including pressure groups within and outside government—would be welcome. Gann and Duignan are not unaware of the importance of this subject. They just don't think it is worth a whole section. Another general theme, suggested by the dialectic of the construction of an imperial order and its overthrow, is the interplay of "allocating values" in the field, in territorial headquarters and in the metropolitan capital. In perpetuating the convention of confining discussion of colonial policy to one Metropole at a time (omitting also, by the way, the experience of Spain and Italy), the editors miss an opportunity to review comparatively what we might call "command and control" problems, as well as a derived issue, intrabureaucratic politicking. Michael Crowder's chapter on "The White Chiefs" makes a start that suggests this line, but it is all too brief.

Finally, except for potshots at a vulgar economic interpretation of political events, the editors relegate mining and commerce to a rather recessive role. Perhaps the fourth volume will make up this loss. Yet inclusion of these forces in the economics volume perhaps prejudges the controversy. Colonial rule as a system of power —the political economy of colonialism—seems weighty enough to risk substantial treatment. In addition, a serious Marxist interpretation of colonialism and revolution might spell out the purported dangers of a doctrinal approach, and provide some raw meat for argument.

The editors make some good suggestions for further research, most of which have political import: the origins of African socialism, cooperation between groups in the black diaspora and African political movements, the influence of neo-traditional symbols on political rhetoric and opinion, and biographies of African political leaders. Possibly, Gann and Duignan think more work has to be done on these subjects before contributions can be invited. My guess is that chapters on these subjects at present could be just as illuminating as the rather thin and Anglocentric contribution on "Military and Police Forces" or the establishment-oriented chapter on "Missionary and Humanitarian Interests."

It appears the editors want their audience to include more than scholars and Africanists, but the style of discussion is exceedingly broad, even for the general reader. It ranges from a kind of undergraduate lecture by Hubert Deschamps ("France in Black Africa and Madagascar between 1920 and 1945"), through a stimulating sketch of greater research possibilities by Roger Anstey ("Belgian Rule in the Congo and the Aspirations of the Evolue Class"), to Kilson's data- and citation-crammed paper fit for a professional gathering. To call the contents "uneven" is an understatement.

More significant, however, is the lack of a sustained perspective or of the opposite—a series of reasonable and enlightening alternative perspectives with which to view the enormous changes wrought by colonialism. Most of the contributors seem unaware of one another's papers, although the attentive reader can find some similar issues with which to create a dialogue. Gann and Duignan themselves, however, do have a perspective, prominent in their "Introduction," their "Epilogue," and their chapter, "Changing Patterns of a White Elite." They see colonialism in the light of historical forces at work on a worldwide scale, forces that involved beneficent motives, processes, and outcomes. Unfortunately their approach is barely reflected in other contributions.

Put rather bluntly, Gann and Duignan are telling us that white interests in Africa were, for the most part, not exploitative and not racist. They believe scholars ought to be brave enough to examine their naive, liberal, reformist biases. It is difficult to predict the overall impact of their multi-volume effort, but it is clear that Gann and Duignan have staked a claim as academic revisionists. (I might add "right-wing," but that would place me in the same doctrinaire bag as many of my fellow academicians.) Gann and Duignan make a useful analytical contribution, although their chapter on the white elite may strike some people as bordering on apologetics. They want scholars to examine policies and the consequences of policies in the light of possible alternatives, seeing actions and outcomes in terms of least-worst options. In addition, they emphasize the two-edged nature of sociopolitical relationships; for instance, at some point dependence can have advantages. The editors make the intriguing suggestion that we view colonialism as a facet in the expansion of "the frontier" around the world. They believe race relations in Africa under colonial rule were no worse than elsewhere and in some cases a good deal better.

Readers might be more confident in their own evaluation of this position if other contributors tackled the issues raised by Gann and Duignan. They might agree that it is time "to strike a balance between the Eurocentric and the Afrocentric approach" (p. ix), if a few more chapters were clearly Afrocentric. They might also feel less uneasy about the whole argument if contrasting views did not invariably set up straw Marxists to knock down, as I fear Gann and Duignan do. Some readers may get the feeling that redressing the scholarly balance fits into some greater design of anticommunist struggle.

This is hardly the definitive work on colonialism in Africa. But it carries within itself the seeds of a worthwhile dispute.

Harvey Glickman

Haverford College

Self-Determination and History in the Third World. By David C. Gordon. (Princeton: Princeton University Press, 1971. Pp. 219. $7.50.)

In the attack upon colonialism it has with good reason come to be accepted both by political activists of the third world and by commentators from many camps and countries that the most grievous injury inflicted by colonialism has been the spread among the colonial peoples themselves of the conviction that they are inferior branches of mankind. The counterpart of this proposition was, of course, the belief among the white, European-descended peoples that their superiority entitled them to dominate the remainder of mankind and to determine the rules by which the great game of world intercourse should be played. The fullest realization of these propositions came in the colonial countries where the presumption was that the rulers acted while the ruled obediently responded.

With the rise of third-world nationalism and the speeding up of decolonization this older relationship became intolerable, and the new rising forces demanded recognition of an equal status and dignity for their peoples. Everywhere as peoples in the modern age have laid claim to their national identity and destiny they have rewritten their history in such fashion as to justify their claim in the light of their distinguished past. On the contemporary scene the most striking case is that of black Africa, striking primarily because of the denial by white historians, who commanded the heights of historiography, and by laymen as well, that Africa had any history other than that imposed by the Europeans. The recent past has seen a rush to fill this supposed vacuum and to demonstrate that Africa had a precolonial history no less worthy of note than that of other parts of the world. In particular the so-called Dar es Salaam school has striven to establish that Africans were not merely passive objects of European initiatives but were from the outset actors in their own right and that the nationalist movements which came to the fore after World War II had firm roots in the early African resistance to European encroachment.

It is this theme, with a concentration of attention on the Middle East and the Arab world, which is elaborated in *Self-Determination and History in the Third World.* In it the author, who is a professor of history at the American University of Beirut, sets out to examine the uses and abuses of history by and on behalf of peoples who are asserting their right to run their own affairs and speak for themselves. The self-determination with which he is essentially concerned is not the legal and political right of "all peoples" to get out from under colonialism —a right that the United Nations has so insistently proclaimed—but rather the search for their own historic souls by peoples who have been excluded from history by Western imperialism.

In the opening paragraph the author distinguishes between two kinds of history. One is that collective memory of a people of its past experiences, heroes, and great deeds which becomes a basis for its sense of identity—"an accumulation of myths, illusions, symbols" (p. 3). The other is the critical, scientific study of scholars for whom history's validity is determined by objective criteria. History as utilitarian myth, serving the collective interest, and history as objective science obviously overlap at a number of points, but however fuzzy the boundary between the two, it is the first type with which Professor Gordon is primarily concerned in this book. Within this type he further distinguishes three different approaches to history of which he makes much use: the approach of the "futurist," who tends to turn his back on history, that of the "apologist," who seeks a return to a specific past tradition, and that of the "resurrectionalist," who looks to a progressive future based on a reinvigorated past.

Sympathetic to the efforts of peoples to free themselves from "the paralyzing myths of the colonizer, if only through countermyths" (p. 191), Professor Gordon also warns of the dangers of a new counter-racialism, citing Paul Valéry's judgment of history as "the most dangerous product evolved from the chemistry of the intellect," making nations "bitter, arrogant, insufferable, and vain" (p. 179). Of the peoples with whom he deals, the author regards the Tunisians as the most "mature" (his own quotation marks) in view of their readiness, following Bourguiba's lead, to acknowledge a broad Mediterranean heritage which enables them to draw upon Rome, Carthage, and France as well as Islam and Arabia.

Two items may be briefly and somewhat arbitrarily selected from the wealth of material the book contains, which, it deserves to be mentioned, includes significant comparative comment on India, Africa, and American black nationalist movements.

Myths not infrequently contradict each other —as the author puts it, one man's myth is another man's poison—and he cites in illustration the conflict between Pan-Arabism and the nationalism of particular Arab peoples. This is peculiarly the case in Africa where any pursuit in depth of national myths means in most instances the singling out of tribes which at least potentially threaten the unity of the incipient nation. Second, the resurrection of the past of the third world to an impressive extent has been a product of the labors of western archeologists, anthropologists, historians, and colonial administrators. The myths and symbols on which the third world draws are only in part, and in a number of countries in small part, the contribution of the people of those countries, although increasingly they are taking over the investigation and presentation of their own heritage.

This is a book which is no doubt of special interest to those concerned with the Middle East, but it is written with style and grace and it develops a theme of universal interest, including inquiry into no less a question than whether history needs to be true.

RUPERT EMERSON

Harvard University

The Containment of Latin America: A History of the Myths and Realities of the Good Neighbor Policy. By David Green. (Chicago: Quadrangle Books, 1971. Pp. 370. $10.00.)

The Quest for Change in Latin America: Sources for a Twentieth Century Analysis. Edited by W. Raymond Duncan & James Nelson Goodsell. (New York: Oxford University Press, 1970. Pp. 562. $12.50.)

In his very readable, well documented work, David Green challenges the generally held view that Franklin Delano Roosevelt's Good Neighbor Policy toward Latin America was a notable success, and the view held by some that the postwar decline in hemispheric unity could be attributed to the abandonment of the economic aid policies of Roosevelt's administration. The Good Neighbor Policy was a success for some years, Green agrees, but he is convinced that over the long run, it proved to be a failure. Indeed, he claims, it was largely because succeeding administrations followed President Roosevelt's economic aid policies that relations between the United States and Latin America have deteriorated to their low point today.

By the time Roosevelt proclaimed the Good Neighbor Policy in 1933, depression nationalism in Latin America had already reached full blast; in fact, revolutionary nationalism was aflame in many of the countries. And although the national movements differed in many respects, all of them had one common concern: their goal could be achieved only if their nations were able to gain full control of their own economic resources, many of which were owned and controlled by foreigners, especially Americans.

Yet ignoring the ominous portents, the Roosevelt administration almost from the beginning took a strongly negative attitude toward rising Latin American nationalism. The administration was certainly not opposed to the development of Latin American resources, for the United States depended upon and profited from their development. Nor was the administration rigidly committed to defending the property rights of Americans in the region. Rather, Green believes, administration policy-makers for the most part "felt that they, rather than the revolutionary nationalists, had a more realistic and sensible perception as to how Latin American resources development ought to proceed" (Preface, ix).

This negative attitude seemed to give way to a more positive one when President Roosevelt announced a "New Approach" in 1940. Referring to Latin America, the President stated, "Give them a share. They think they are just as good as we are, and many of them are" (p. 38). The New Approach at first seemed promising, for it recognized the need both of American private enterprise and of Latin American societal growth. But World War II served to maintain and even strengthen United States influence over the nature and direction of Latin American economic development.

Green's work, utilizing many unpublished as well as published materials and also interviews with prominent Americans and Latin Americans who had a hand in the policy making, concludes with 1948. But a brief epilogue of events to date forcefully states his conclusions regarding the long-term effects of the Good Neighbor Policy. Over the long run, he is convinced, the Good Neighbor Policy was a failure. For the establishment by the Truman administration of the Inter-American system in 1947 and 1948 in effect formalized United States supervision and control of Latin American economic development. Also, the policymakers, besides failing to convince Latin Americans that the Good Neighbor Policy was a policy of institutionalized benevolence, more importantly failed to dissipate or obviate the need for rampant Latin American revolutionary nationalism.

As the United States became more involved directly or indirectly in pushing through its policies, it became obvious that the Good Neighbor Policy, stressing a closed hemisphere in an Open World, had resulted in a plan for containment of Latin America. Ironically, the policy in time even equated revolutionary nationalism in Latin America with the supposedly dangerous threat of communism.

Moreover, the Good Neighbor Policy was largely responsible for the political instability prevalent in Latin America. For while economic control remained frozen in the hands of foreigners, "political control of local government machinery in Latin America passed swiftly from hand to hand among various local groups, all competing for the wealth and power which could be theirs through 'cooperation' with the United States on an inter-governmental basis" (p. 295).

In essence, Green contends, the United States did not want neighboring allies who were "dependable *and* strong" but allies who were "dependable *and weak*" (p. 296). In explaining this stance, Green's commendable work should be a choice morsel for policy-makers and others to chew on. It is hoped that the State Department has some dog-eared copies it is putting to good use.

A Quest for Change in Latin America, edited by W. Raymond Duncan & James Nelson Goodsell, attempts to provide authoritative sources for an in-depth understanding of the ceaseless, militant striving for change that pervades Latin America today: political change, economic change, social change. Changes in every realm of the developing society that will enable it to utilize its full potential, realize its deep aspirations, and attain its desired stature in the world.

Of the 89 well selected readings, some are documents, such as Articles 27 and 123 of Mexico's Constitution of 1917, and the Objectives of Bolivia's Agrarian Law promulgated in August, 1953. But most of them are the speeches and writings of influential political spokesmen who played a prominent role in the development of their own countries or of Latin America as a whole.

The tone of rising, militant nationalism is explicit or implicit in most of the selections. Almost half of the volume (three of the seven sections) is devoted to nationalism in three nations—Mexico's Revolution of 1910, Cuba's Revolution of 1959, and Chile's "Revolution in Liberty" of 1964. The various spokesmen and writers give vent to their ideas and feelings on a wide variety of controversial issues, such as the North American threat, the European threat, anticlericalism, land reform, and Yankee capitalism. A brief introductory note preceding each reading gives the setting and helps to interpret it.

Yet whatever the subject of a particular speech, article, or document, the underlying idea is how to achieve change. For as the editors state,

> Since the opening of the century, there has been a growing acceptance by Latin American leaders of the idea that man makes and is, at least in part, responsible for his own society. That idea is the common denominator of the sources brought together in this book. The acceptance of the idea brought about a willingness to change that could well be compared to the radical nature of the Copernican Revolution (Preface, vii).

The many ideas expressed in the volume are a yeasty brew and will probably stimulate the student of Latin American politics to probe more deeply into the philosophies and contributions of the various leaders. Especially interesting are the great differences of opinion expressed by the leaders on significant issues. What should be the national goals, and which should receive priority? What are the best means of attaining them? Just how fast should the rate of change be? And perhaps most important of all, should needed change be accomplished by revolution or by reform?

This stimulating collection of readings, many now available for the first time in English, is a most worthwhile contribution to a realistic understanding of what Latin Americans want, why they want it, and why they will eventually get it.

ALEXANDER EDELMANN
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The Study of Coalition Behavior: Theoretical Perspectives and Cases from Four Continents. Edited by Sven Groennings, E. W. Kelley, and Michael Leiserson. (New York: Holt, Rinehart and Winston, 1970. Pp. 489. $9.95.)

It has been nearly a decade since William Riker first demonstrated in *The Theory of Political Coalitions* (New Haven, Conn.: Yale University Press, 1962) the theoretical leverage that a focus on coalitions can offer to the study of politics. For many studies in the volume under review, Riker's rigorous development and fruitful application of formal methods to the study of coalition behavior serves as a theoretical benchmark. Now, however, "the theory" in the title of Riker's book has become "theoretical perspectives" in the title of *The Study of Coalition Behavior,* aptly reflecting the diffuseness of coalition theory today. Unfortunately, the editors of this volume have not brought much coherence to these perspectives and have been derelict even in failing to publish an index, which makes it very difficult to trace common themes through the volume's twenty-five chapters.

More serious problems arise, however, in the "basic definitions and assumptions" they posit, which do more to confuse than clarify issues. By stating what they do *not* assume at the outset, the editors admirably fulfill their purpose of providing "as much variety as possible" (p. 3)—and giving almost total indirection to what follows. Lest this theoretical leeway turn into theoretical license, however, we are told that coalitions "are seen as events in a process" (p. 7). Whatever *this* means, we can at least ruminate on the proposition that this notion of coalition "is neither static nor conventional" (p. 7). Of course, conventional thoughts about a field "still in its infancy" (p. 3) would be unnatural.

When the editors finally get down to basic definitions (at least those which are italicized), they first eliminate from consideration *coalition situations* that produce "maximum satisfaction for all the actors" (p. 7). I think this is an unfortunate restriction, because it not only excludes the study of conditions that lead to Pareto-optimal outcomes but also seems to exclude the study of public goods. Such goods by definition are available to all members of a group and provide satisfaction which is maximal in the sense that no individual could purchase more only for himself.

As Riker has given major impetus to the study of coalition behavior in conflictual (i.e., predominantly zero-sum) environments, Mancur Olson in his *The Logic of Collective Action* (Cambridge, Mass.: Harvard University Press, 1965), has done so in environments not assumed to be conflictual. I think Olson's study and the work of others in the area of public goods genuinely address how and under what circumstances coalitions form, and they do not deserve to be excluded from studies of coalition behavior. (It should be noted that in eliminating conflict from the environment, Olson does not assume it out of politics but instead postulates it in the form of the "free-rider" problem, which occurs when individual members of a group do not contribute to the purchase of a public good from which all benefit.) To be sure, the editors may limit their domain of inquiry to whatever they please, but from the several objections raised by contributors in this volume to Riker's zero-sum assumption, a number would seem to agree with the position taken here about the utility of other assumptions about the nature of the environment in which coalitions form.

Contrary to the claims of the editors, many of the contributors very definitely do not subscribe to their definition of a *coalition* as "two or more actors in a coalition situation who have communicated and agreed to coordinate their actions" (p. 7). It is simply beyond belief that the scores and even hundreds of members of legislatures, political parties, and other elite groups considered in the twelve case studies—if these members are taken to be the actors in the above definition—have communicated with each other, much less agreed to coordinate their actions. To avoid limiting studies of coalition behavior to small groups, I would suggest that a more fruitful definition of a coalition might be "those actors or sets of actors with complementary interests who join together to form a group that exacts some payment (not necessarily monetary) from its members."

I have dwelt on the editors' introductory remarks because (1) it is impossible in the space provided to review all the contributions, and (2) the direction (or lack of it) which the editors give to such a volume is crucial in evaluating its contents. Since the contributors to this sundry collection do not seem to have adhered to what common terminology and assumptions the editors did offer, I shall evaluate the contributions for which space remains independently of the guidelines the editors claimed to have imposed.

Roughly the first half of this volume is devoted to empirical case studies, several of which are excellent. Especially impressive are the systematic compilation and analysis of quantitative data on legislative and electoral coalitions in West Germany since World War II by Peter Merkl, on electoral coalitions in the 1951 and 1956 parliamentary elections in the Fourth French Republic by Howard Rosenthal, and among factions of the Liberal-Democratic Party in Japan since 1955 by Michael Leiserson. In the case of the latter two contributions, I found most enlightening Rosenthal's use of prior and posterior estimates in testing propositions about the size of coalitions, and Leiserson's candid discussion about the potential shortcomings of a focus on coalitions. Some useful refinements in Riker's theory are suggested by Phyllis Peterson's test of the size principle using local-election voting data from São Paulo, Brazil, and by Gerald Pomper's imaginative historical analysis of coalition-formation processes in the American Constitutional Convention of 1787.

In offering qualifications to Riker's size principle, several contributors to this volume show how other factors, like the desire to minimize ideological distance or the number of members in a coalition, may improve the predictive power of his theory and may also lead to other predictions, such as what the composition of coalitions and the distribution of payoffs (e.g., cabinet posts) among coalition members (e.g., political parties) will be. Unfortunately, most of these other factors adduced to explain coalition outcomes are not derived from the assumptions of a model and hence have a largely *ad hoc* flavor. An exception is Abraham De Swaan's contribution, but even his careful if informal derivation of consequences from a set of assumptions has a built-in circularity: the ideological compatibility of actors (derived from an ideological scale along which he assumes actors can be ordered) is determined from preferences that presumably are in part based on previous party alignments, which in turn are used to postdict what ideologically compatible coalitions will form.

If ideology vitiates the operation of the size principle by precluding actors of dissimilar beliefs from joining the same coalition, it might be argued at the same time that ideological cleavages increase the perfectness of information, and thereby the applicability of the size principle, by making choices more predictable. Clearly, such plausible yet divergent claims made for the effects produced by ideology on the size principle leave its status as an explanatory variable unsettled. This ambiguity vividly illustrates the kinds of logical problems that can be created by the introduction of new "explanatory" factors in a model. These factors may lead to good predictions but, at best, confused explanations.

In some cases it is not clear what distinguishes "cases from four continents" in the first half of the book from "theoretical and methodological perspectives" in the second half of the book, which also includes reports (like De Swaan's contribution) on empirical findings. Logical and empirical analysis merge in Dina Zinnes's argument that Riker's theoretical assumptions are empirically untenable when applied to the study of balance-of-power systems. This argument is hard to evaluate, principally because it is so difficult to determine whether the assumptions of a theoretical model like Riker's are "true." Not only is the usefulness of a model often quite independent of the realism of its assumptions, but in testing a theory it is generally more productive to test the consequences of a model's assumptions rather than the assumptions themselves. More to the point, however, is that Zinnes's contention is based on empirical observations; it does not really come to grips with Riker's logical demonstration that there is an internal inconsistency in the balance-of-power rules of Morton Kaplan. If one accepts Kaplan's rules as descriptive of balance-of-power systems, then one must accept Riker's conclusion that such systems are inherently unstable.

For political scientists interested in the insights other disciplines may offer to the study of coalition behavior, the review articles on game theory by Leiserson, on economic bargaining and utility theory by E. W. Kelley, and on sociopsychological theories and research by Jerome Chertkoff are informative, as are some of the articles I have not discussed. Although I can commend several individual pieces in this volume, the collection as a whole suffers from a lack of editorial integration and logical organization, the latter I'm afraid being in part a function of the present state of the art.

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Internal Colonialism and Structural Change in Colombia. Edited by A. Eugene Havens and William L. Flinn. (New York: Praeger Publishers, 1970. Pp. 252. $15.00.)

Like many before them—from Marx to the originators of project Camelot—Havens and Flinn are concerned with the potential for revolutionary change. One of the editors' two "central assumptions" is that an examination of Colombia, a country with many variations, will allow the formulation of insights about change that can be applied to the Latin American region as a whole.

The other "central assumption" sets the tone for much of what follows: "It is the editors' contention that institutional change must take place before economic growth will be self-sustaining in Latin America; that change in the economic structure is a necessary condition for societal development, since economic relations directly affect other social relations." It is clear from later passages that the type of institutional change the authors see as necessary is not evolutionary, since institutional change is equated with "structural change"—an "alteration in the organizational arrangements of a total society." Thus, Colombia needs a thorough-going revolution.

A predictive note is added to the prescriptive orientation: "Eventually such a revolutionary change will occur and it will be the result of a certain set of processes and of organized activity." The reader is promised a documentation of the social changes occurring in Colombia and the accompanying pressures on the system as well as an analysis of the social processes leading to revolutionary change (p. 16).

This is a Herculean task—especially for an edited book only 250 pages long—and it is not surprising that the editors fall short of the mark. In the summary chapter, in which the analysis is to take place, eleven descriptive statements are offered, and of the eleven, six at most deal with *changing* conditions: a decline in economic growth indicators for major sectors of the urban, industrial economy relative to earlier periods; an "increasing man-land ratio" in the rural areas and heavy urban migration; "constant or declining employment opportunities"; normlessness on the part of economically marginal groups; a growing gap between the standard of living of large and small farmers and the production of latifundia in new areas of colonization. But there is no attempt to integrate such findings into an "analysis of the social process" preceding the inevitable structural change. In fact, some of the findings themselves are either questionable (e.g., there is no convincing systematic evidence for the "normlessness" proposition) or require statistics to establish the magnitude of the problem (e.g., standard of living data).

Although "internal colonialism" appears in the title, it is not given extensive treatment in the body of the work—in fact it is rarely mentioned. Rather, it also is an "assumption" and "is not subjected to direct empirical examination." We are instructed that the "validity of the assumption can only be inferred from the analysis that follows." But given their definition of internal colonialism—an arrangement characterized by the presence of a small, dominant group of landowning capitalists versus a "large, subjected mass" with little or no mobility or participation—and the kinds of materials presented in the volume, such an inference is difficult to make. In fact, one of the chapters dealing with the occupational mobility of middle groupings (Chapter 13), tends to confuse the reader: Where do the middle segments fit into the two-class frame?

At least two additional disharmonies exist between the Havens-Flinn prescriptive macrosociological orientation and the materials in the body of the book. First, many of the data are based on microstudies and hence make generalization difficult (see Chapter 6: "The Power Structure in a Shantytown" and Chapter 7: "Urban Lower Class Voter Participation and Political Attitudes"); this problem, of course is one that many of us have faced, with varying degrees of success.

Second and more significantly, some of the chapter materials, while prescriptive, are incongruous with the editors' deterministic, revolutionary frame. The two chapters by Dale Adams and Gustavo Jiménez's chapter on the Church best illustrate this. In Chapter 8 ("The Land-Tenure System: Antecedents and Problems") Adams acknowledges that governmental efforts are being made to "further accelerate changes" in the land tenure system and that the structural (politico-legal) obstacle is only one of many. He modifies the two-class latifundia/minifundia dichotomy by noting many different types of each and maintains that the importance of the land concentration problem has been overstated. In Chapter 11 ("Leadership, Education and Agricultural Programs") Adams notes that one of the evolutionary reforms he emphasizes has been accomplished in "at least one rural area" (p. 195). Nowhere does he indicate a necessity for (or sympathy with) total revolution. Finally, Jiménez's materials confirm the Church as mainly a status-quo institution, but are not set in the context of revolutionary determinism.

If the book suffers from problems at the "design" level, the individual chapters provide some valuable insights, especially into the situation of the disadvantaged members of Colombian society. Harlan Davis concludes that the scarcity of modern public services in the countryside is due to the absence of "effective mechanisms to press the changes" and that poverty is partly responsible. But another cause of the situation is the "lack of autonomy and decision-making authority at the local level" (p. 82).

Joseph Thome elucidates well the problems caused by title insecurity—inability to obtain credit and problems in the transfer of land are two of the most serious. Havens and Flinn had concluded in a previous chapter that the subsistence farmer "receives little, if any, assistance from the introduction of new farming techniques" and that this "is partially due to the subsistence farmer's perceived inability to tolerate the risks involved if the new practice should fail" (p. 43). When these observations are added to those of Thome, it is not difficult to appreciate the adverse conditions experienced by the small farmer.

In the Havens-Flinn chapter on education (among the best documented) it is likewise easily concluded that access to rural instruction, especially at the secondary level, is limited to a small minority of school-age children. The authors attribute limited opportunities in education to "structural blocks" but fail to show why it is simply not the result of a paucity of national financial resources.

In his chapter on the "Occupational Mobility of the Middle Strata" Rodrigo Parra sets out two major aims: to analyze the "role these [middle strata] groups have played in changing the occupational structure of Colombia" and to "examine the manner in which Colombian society has influenced the development of the new middle strata" (pp. 215–216). That he does not make these analyses the focal point of his conclusions is obvious even on superficial reading. Parra does, however, confirm for Colombia what others have pointed out and analyzed for Latin America as a whole: that urbanization is not accompanied by proportionately increasing industrialization and that great increases in tertiary occupations are a result.

But as an entity, this book is disappointing.

By starting out prescriptively, it runs the risk of a double defeat. First, a more objective "scientific" orientation, the most sound basis for prescription, is discouraged. Thus, Adams prescribes more people with rural backgrounds in agricultural leadership programs without first trying to establish whether in the group he studied there were indeed significant differences in the behavior and attitudes of rural versus urban agricultural leaders. Second, the authors of prescriptive analyses are usually striving for policy impact. True, if the revolution comes, this book could conceivably play a part. But since Latin American nationalists and political influentials of varied stripes seem increasingly less willing to follow the advice of foreigners, the political effectiveness of this volume is likely to be minimal.

REID R. READING

University of Pittsburgh

Communists and Their Law: A Search for the Common Core of the Legal Systems of the Marxian Socialist States. By John N. Hazard. (Chicago: University of Chicago Press, 1969. Pp. 560. $8.75.)

Professor Hazard has undertaken the gargantuan task of comparing the legal systems of the fourteen countries of the world that are ruled by Communist Parties, namely, the Soviet Union, its six fellow-members of the Warsaw Pact, Yugoslavia, China, North Korea, North Vietnam, the Mongolian People's Republic, Albania, and Cuba. The purpose of the comparison is to discover the basic common elements in "the family of socialist legal systems."

The starting-point is the 1957 Declaration of the Twelve Communist Parties in Power, which claims to lay down "basic laws applicable in all countries embarking upon a socialist course" (p. 6). These are (1) leadership of the Marxist-Leninist party, (2) state ownership of the basic means of production, (3) gradual Socialist reconstruction of agriculture, (4) planned economy, (5) a Socialist revolution in ideology and culture, (6) friendship among nationalities, (7) defense against external and internal enemies, and (8) proletarian internationalism. Professor Hazard asserts that the legal systems of the fourteen countries under consideration are designed to achieve these goals. He recognizes that "there is room for considerable variation in the choice of means . . . and the choices of the fourteen Marxian socialist states have been far from uniform . . ." (p. 8). Nevertheless, he believes that there is a "common core" and that the fourteen legal systems belong to the same "family."

To test this proposition, various basic aspects of the socialist legal systems are examined in turn: Constitutional and administrative provisions affecting the position of the leadership at the higher and intermediate levels; concepts of the nature of law and its relationship to the masses of people; land ownership and utilization; state ownership of production enterprises; individual rights of property, contract, and inheritance; family law, tort law, criminal law, and others. The method of examining each aspect is, first, to focus on the Soviet legal system,

> since that is the inspiration for the others within the family. Materials taken primarily from the Polish, Yugoslav, Czechoslovak, and Chinese systems are then introduced, along with lesser quantities of data from others within the family, to provide the basis for generalizations about the common core (p. xii).

The author has his greatest difficulty in fitting China and Yugoslavia into this scheme. He states that they are at opposite ends of the spectrum of socialist law (p. 8), but again and again their legal systems appear to fall entirely outside his criteria. He seems to be on firmest ground in asserting that all fourteen states have systems of public law that rest on the foundation of a one-party system (p. 27), and that preservation of the leadership of the Party is "a cardinal point of the 'style' of the Marxian socialist system, if not *the* cardinal point . . ." (p. 33). Yet even here qualifications are needed. Stalin replaced party rule by the rule of the secret police, and apparently Mao replaced it at one time by the rule of the army (cf. p. 65). Moreover, it is doubtful that party leadership means the same thing in the Soviet Union, China, and Yugoslavia.

In other areas the diversities are even greater, so that what are described as the same broad goals or principles—"state ownership of the means of production," "planned economy," "collective use of the land," etc.—are only the same if they are viewed in isolation from the very different kinds of legal institutions used to implement them. The principle of "state ownership," for example, may be said to exist in two countries with different operative concepts of ownership, but then we are talking about two principles, not one. By analogy, one would hesitate to speak of "private ownership" and "market economy" as common principles of two countries with such diverse legal systems as, say, the United States and Pakistan. Is there a "family of capitalist legal systems"?

The difficulty is compounded by Professor Hazard's demonstration that in the area of private law—individual rights of contract, property, and inheritance, family law, etc.—the Marxian socialist states have returned, for the most part, to traditional (i.e., non-Marxian) doctrines; and further, that the emphasis upon legal flexibility, simplicity, and popularity in the early stages of socialist development have been superseded everywhere—except in China—by an emphasis upon stability, formalism, and professionalism. Thus the principal distinguishing features of the socialist legal systems are said to be confined to certain areas of public law. Yet there is hardly a legal situation—in any system—in which public law and private law are not closely interrelated.

It is submitted that a quite different picture of socialist law would emerge if instead of comparing the legal systems of fourteen states, one were to compare the legal systems of two—say the USSR and China, or the USSR and Yugoslavia, or the USSR and Poland or Czechoslovakia. Then one would have to take greater account of historical factors as well as of political circumstances, social and economic conditions, etc. The "Marxian" features would then be seen in a better perspective.

This is not to say that the search for the distinctive elements shared by all Communist legal systems is not worth undertaking. On the contrary, we can only be grateful to Professor Hazard, the dean of Soviet legal studies in the United States, for turning his abundant energies to the task he set for himself. His book is one that every student of Communism should read, even though he may come away, as this reviewer did, with more questions than answers.

HAROLD J. BERMAN

Harvard University

God's Englishman: Oliver Cromwell and the English Revolution. By Christopher Hill. (New York: The Dial Press, 1970. Pp. 324. $7.95.)

The Character of the Good Ruler: A Study of Puritan Political Ideas in New England, 1630–1730. By T. H. Breen. (New Haven: Yale University Press, 1970. Pp. 301. $10.00.)

Here are two studies in the erosion of religious zeal and the secularization of political life. Of the two, *God's Englishman* is by far the more interesting, not only because it deals with greater men and more important events. Christopher Hill has in fact written an excellent biography of Cromwell. But the special value of his book for students of politics lies in the way he fixes on, worries about, and analyzes the English Thermidor; that is, the rightward drift of the revolution after the execution of the king. Cromwell, who had pressed for the king's death ("I tell you, we will cut off his head with the crown on it"), is at the same time the great English Thermidorean. Though the return of England's "natural rulers" is not something he plotted, as some of the Levellers thought, the tale of that return might well be told in terms of his hesitations, enthusiasms, and disillusionments. It might also be told in terms of those objective conditions that set limits to what any revolutionary can achieve. Hill tells both these stories: his book combines conventional biography and Marxian analysis, and while he has no new research to report and no new theory of the revolutionary leader, the combination brings us quickly to the heart of the dilemmas such a leader always and perhaps necessarily faces.

"Underlying all the disputes between Oliver Cromwell, the Levellers, and his parliaments, was one fundamental problem: the problem of the electorate" (p. 206). It is, one might say of a less "constitutionalist" polity than England, the problem of a social base. Neither the political nor the religious radicals (and Cromwell was allied with both at different times in his career) had the support of a sufficient number of Englishmen. There was never a majority for democracy, toleration, or godliness in the seventeenth century, either among the propertied classes or among the mass of the people. For all his sympathy with the Levellers, Hill sees clearly enough that had they seized power, they would have "faced the necessity of holding the rest of the population down" (p. 207). The point is equally true of the Puritan "saints" of the Barebones Parliament, who could not rule without the army, and of course, it is true of the officers themselves, the very embodiment of what Andrew Marvell called "the forced power." Hill does not mean to say that England's "natural rulers" could govern the country without violence, but they required a great deal less of it than any alternative group, and ultimately any government which did not intend systematically to destroy them, had to make its peace and admit them to power.

The crucial moment for Cromwell, Hill rightly argues, was not the defeat of the Levellers at Burford, but the dissolution of the Barebones Parliament. God's people, and not the people simply, were always the basis of his political faith, but God's people were at war with one another and with the propertied classes, and these were not wars in which Cromwell found any providential guidance. After 1653, he no longer dreamed of the triumph of the saints: "he saw himself as a constable whose task was to prevent Englishmen from flying at one another's throats" (p. 143). His it was, finally, to defend law and order—and property. It is the same task that John Locke only a few years later described as the central and virtually the sole function of government.

T. H. Breen's book traces a hundred-year-long movement "from piety to property" in the writings of New England Puritans. He is, then, concerned with developments directly analogous to those which took place in England but, because of the heightened tension and accelerated pace of revolutionary politics, in a much briefer time span. The analogy is not one that he discusses, however; nor does he have much to say about the movement as a general phenomenon; nor, again, is he given to speculation as to its possible causes. His is a resolutely conventional history; it is always careful and it is highly competent; but it must appear to social scientists as a record of missed opportunities. Though he retells some of the political history of New England and grants that, in this case at least, "theory seemed to joggle along behind practice," he is content mostly to joggle along behind theory. And so he offers us no insights into the complex and interconnected character of the secularization process.

Indeed, it is hard to make out from this book just what secularization is: what precisely is it that survives, what is it that disappears or is transformed in religious thought? More concretely, when a study of Puritan ideas ends with a consideration of the Franklin brothers' *New England Courant,* is its author pointing to the death of Puritanism or to its survival in strange, distorted, partial, in a word, *secularized* forms? Weber thought one needed Puritanism to account for Benjamin Franklin's economic thought, but is the faith of John Winthrop central to an understanding of *Cato's Letters?* Perhaps it is, but Breen does not tell us that it is, or why. Only once does he address himself to this sort of question—at the end of his fifth chapter in a summary statement that is worth quoting at length:

> New Englanders, Breen writes, "intermixed the language of property and morality, as men did not in England; and because of this blending, colonial attitudes about government acquired a unique character. The citizens came to see any invasion of private estates as sinful acts prompted by the magistrate's impiety; and during the American Revolution the mingling of material and spiritual values led Puritans to condemn taxes levied by the idle, sumptuous rulers of Britain as both arbitrary and immoral (p. 202).

That is an important point, and a book which argued it at length and in detail (and with the necessary comparisons) would be an important book. It is not the book Breen has written.

MICHAEL WALZER

Harvard University

The Politics of Reform in Peru: The Aprista and Other Mass Parties of Latin America. By Grant Hilliker. (Baltimore and London: The Johns Hopkins Press, 1971. Pp. 201. $10.00.)

Prior to the mid-1960s, few monographs dealing with Peruvian politics had appeared in English. Then in 1965 came Payne's *Labor and Politics in Peru,* followed in 1969 by Larson and Bergman's *Social Stratification in Peru* and Astiz's *Pressure Groups and Power Elites in Peruvian Politics,* and in 1970 by the English translation of Bourricaud's work of four years earlier, *Power and Society in Contemporary Peru.* Mr. Hilliker's study of the Peruvian Aprista Party—the subject of this review—draws deeply on all of these, as well as on much of the now considerable literature in Spanish.

The Peruvian Aprista Party has been chosen as an example of a "mass reform party" (p. 11), as well as one of the earliest parties of the so-called "Democratic Left" in Latin America. Chapter VIII specifically includes comparative information on the Bolivian National Revolutionary Movement (*Movimiento Nacional Revolucionario*) and the Venezuelan Democratic Action (*Acción Democrática*), two other mass reform parties; but the body of Hilliker's study focuses on the Peruvian Aprista Party or APRA (*Alianza Popular Revolucionaria Americana*). After two introductory chapters, the reader is presented with chapters on Aprista party history, party organization, tactics, and the party's involvement with the agrarian reform issue. In most of these chapters, the author attempts to relate the past activity of the APRA to the process of "national development" in Peru.

The Politics of Reform in Peru was written by a Foreign Service Officer whose work reflects the ideology and rhetoric of the New Frontier and the early Alliance for Progress. As a self-described "twentieth century liberal" (p. 187), Mr. Hilliker quite openly expresses his preference for the non-Marxist "Democratic Left" in Latin America and for the politics of reform rather than the politics of more radical change. Somewhat unfortunately, at times he allows himself to get bogged down in rather vague discussions of "development" without ever succinctly defining what he means by the term. A far more serious omission is his failure to show how the concept of development really helps to improve the reader's understanding of the APRA. While discussions of the "politics of development" have become quite fashionable in recent years, such discussions ought to be reserved for cases in which they have something special to add.

This reviewer finds Mr. Hilliker's characterization of the Peruvian APRA as a "reform party" a debatable view. Little overt mention is made of the apparent conservatism that has affected APRA in recent years. Despite its reformist program of 1931, which never has been formally abandoned, the party has not shown itself through its actions to be greatly concerned with current social reform. The support given by the APRA to the conservative government of Manuel Prado between 1956 and 1962, and the party's parliamentary coalition from 1963 to 1968 with the right-wing National Odriista Union (*Unión Nacional Odriista*) of ex-dictator Manuel Odría could be interpreted by an unfriendly critic as political opportunism. The strategies employed by APRA since 1956 might be justified by the party's need to accomodate the extremely hostile conservative elements in the military which previously had blocked APRA's way to the key office of the Presidency; but such an explanation will find doubters both in Peru and in the United States.

On a more positive note, Mr. Hilliker should be complimented for his diligence in using the available secondary literature on Peru. While not every reader will agree with all of the author's interpretations of this literature, such differences of opinion probably are inevitable in any study. The author's narration of the APRA's most recent periods of legality (1945–1948 and 1956 to the present) represents a highly plausible account of the political events of those years as they have affected the party.

The useful summary of the literature found in *The Politics of Reform* well might have been supplemented by more original data than is found in the text. The limited interviewing that was done adds little to the material now available elsewhere. In particular, this reviewer would have liked to have seen an analysis of the internal workings of the party and how the divisions in the top leadership have reflected the party's various choices of tactics in the past. The chapter on organization (Chapter IV) really only describes in broad generalities the organizational framework of the party. Nothing is said about the leadership except to mention the position of party founder and *Jefe,* Haya de la Torre, now in his seventies. What of the men below him who have had an important say in day-to-day operations of the party during Haya's prolonged stays abroad? What about the party's future? No attempt has been made to describe how the military regime now in power may be undercutting APRA's base of support in the Aprista "solid North" with the *junta*'s own agrarian reform program.

In summary, Grant Hilliker has written a book describing the APRA Party, with considerable attention to its past attempts at winning power in Peru. The material added in the penultimate chapter on two other "mass reform parties" places the Peruvian data in a limited comparative focus. If the reader seeks a competent analysis of the political history of the APRA, he should be happy with Hilliker's book; if the same reader also seeks new data on the party or a provocative analytical framework, he will be disappointed.

EDWARD C. EPSTEIN

University of Utah

The Cameroon Federation: Political Integration in a Fragmentary Society. By Willard R. Johnson. (Princeton: Princeton University Press, 1970. Pp. 426. $12.50.)

Early in 1961, citizens of the Trust Territory of the Southern Cameroons backed into one of the oddest federations on the contemporary political map. The resultant Federal Republic of Cameroon exhibits all the usual splinterings of tropical African states: ethnic diversity; contrasts between "bush" villages and glittering capital; juxtaposition of traditional subsistence farming, plantation agriculture, and major extractive industry; arid steppe and rain-soaked forest. Even beyond these contrasts, Cameroon adds others. Alone among tropical African states, it boasts two official tongues, three former colonial overlords, and a federal union that resulted to a substantial extent from decisions of the United Nations General Assembly.

With only a decade of independent and federated existence, Cameroon can scarcely claim to be a mature political union. The integration with which this study is concerned is one of process, not one of conclusive result. Professor Johnson aptly deems Cameroon a "fragmentary" society—not fragmented, since its current structures are building toward unity. The harmonization achieved to date, however, has levied its main toll upon the former Southern Cameroons (now West Cameroon), whose political processes gradually are being integrated with—or, perhaps more accurately, into—those of formerly French-administered East Cameroon.

Professor Johnson sees integration as "the process whereby political interaction becomes more systemic." I stumbled over this wording. Why not "more systematic"? Since a system is by definition total and englobing, an action can become only systemic, not "more" systemic. This quibble points to a broader problem. The weaknesses in Professor Johnson's treatment stem not from his detailed understanding of a fascinatingly complex society, but from what I am tempted to call the unintegrated study of political integration. Is political integration most fruitfully conceptualized as a process? Or as a goal, reached more or less completely? Should the scholar's perspective be fixed upon a would-be "integrated" system that has yet to come into being? Professor Johnson prefers this last possibility, since his theoretical focus is upon interdependency. Such interdependency is now only germinating in Cameroon.

The theoretical framework of *The Cameroon Federation* incorporates complementary and conflictful political actions (most notably the pursuit of political objectives on the basis of common rules), conjunctive and disjunctive political actions ("weaving behaviors of each citizen into the fabric of national political life"), and the mix of complementarity and conflict within, and among, the various political processes of the federation. The factors thus are complex, varied, and at times confusing. Professor Johnson shifts from individuals to institutions to data on economic interaction, with transitions that are occasionally jagged. What emerges clearly are the facts that one of the two federal partners has borne the brunt of adjustment; that an "integrative advance" has occurred within a decade as the result of implementing a Federal constitution giving restricted authority to the two federated states; and that unification had been given little serious attention by either partner prior to the United Nations-mandated plebiscite.

Professor Johnson rebuts the usual factors that scholars of political union ascribe to successful integration (however "successful" or "integration" may be defined). Ethnic affinities were insufficient to constitute a basis for national political awareness and a coherent political process. The dim memory of German administration, though central to the rhetoric of "Kamerunian" nationalism, did not provide other than a verbal foundation for a "real nation." Language, education, and religion differed markedly in the British- and French-ruled sectors. No manifest economic advantages seemed likely to accrue from unification. Even a "pan-Kamerun" organization to press for unification did not emerge prior to the plebiscite, because most political leaders in Cameroon saw reunification as a tactical maneuver, not a broad strategy.

In point of fact, the formation of the Federal Republic of Cameroon resulted in substantial measure from the wording adopted by the General Assembly for the 1961 plebiscite. Union was imposed upon the inhabitants of the British-administered Trust Territory, who could choose between association with Nigeria or federation with the then-Republic of Cameroon—and could not select the separate independence favored by many. It was, in Professor Johnson's words, the "autonomous power of the mediating agency" that made the Federation's birth possible.

Given the limited extent of prefederation integrative ties, the "integrative advance" is indeed impressive. But it has been an advance felt largely by the smaller, weaker partner in the new Federal Republic. The distinctive role of the United Nations would seem to have few parallels in the annals of political union. Hence, generalizations from the Cameroon example must be drawn with caution. Professor Johnson is appropriately chary about sweeping statements. He concludes with a remark applicable to all splintered, fragmentary African states: the apparatus of state can be built more rapidly than the appurtenances of nation. Given time and wisdom, there is an "even chance" that the "One Cameroon Nation" will prove that a Cameroonian political system has emerged.

CLAUDE E. WELCH, JR.

SUNY at Buffalo

A Year Is Eight Months. By Journalist M. Introduction by Tad Szulc. (Garden City, N.Y.: Doubleday & Co., Inc. Anchor Books Edition, 1971. Pp. xi, 260. $1.95, paper.)

The Czechoslovak Experiment, 1968–1969. By Ivan Sviták. (New York and London: Columbia University Press, 1971. Pp. 243. $10.95.)

The great awakening in the political life of Czechoslovakia and its tragic aftermath was one of the larger issues of 1968 on the world political scene; it produced drama and even ecstasy; it also produced important lessons in the dynamics of international politics of our time. Judged by the latest outcome of events, the bold and ambitious undertaking to freshen the springs of Czechoslovakia's life and to bring about needed reforms is no longer a live issue. The perception and interpretation of the 1968 events, however, is a subject worthy of reflection and analysis.

Writing with the flair and commitment of dedicated reformers, and the insight of knowledgeable participant-observers, Journalist M and Ivan Sviták present to the English-reading audience two differing Czech interpretations of the political currents in their native country.

M's opinions and views are less critical of Czechoslovakia's leaders in 1968 and more approving of the main streams of action. Sviták, on the other hand, from the beginning to the end of the windy Prague Spring shows consistently anti-establishment views, harsh judgments, impatience with the country's leaders, and a strong preference for coping fast and radically with a wide range of social and political issues.

M's book illustrates the dilemmas of reformers working in a rapidly changing political climate, urging moderation, and looking at the political landscape with a certain degree of prudent optimism. Sviták's volume illustrates the dilemmas of political dissidence.

The author of *A Year Is Eight Months* (originally published in German as *Die Kontrollierte Revolution*) is identified in an introductory note by Tad Szulc as "a true-believer Communist" and "a highly active and influential member of the Prague newspaper fraternity" during the Dubcek era. His book is an informative and a very readable work portraying well the unprecedented political activity in Czechoslovakia of 1968. He takes us to the swiftly changing political stage to see real action and to appreciate the importance of the tragic element in the affairs of nations, and he leaves us with the message that hope goes on.

In M's interpretation, three main causes of tension and conflict gave rise to a revolutionary change: economic stagnation, nationalist grievances of the Slovaks, and the suppressed rights of the citizens. The policy of repression could not arrest the progressive decay of the whole system. Under Antonin Novotny's tutelage, the Communist Party was leading the country into a blind alley. Hence, the upper echelons of Party bureaucracy approved the plot to unseat Novotny and to end his regime of personal power. M describes the ideological ferment, the maneuvering of the Party leadership and its apparatus, the political breakthrough against dogmatism and conservatism, and the emergence of Alexander Dubcek's "socialism with a human face." But the ferment gave rise to confusion and disorientation among Party members. The disarray was reflected in the district Party conferences and the meetings of the higher echelons. Three strong and influential groups formed within the Party, each with interests and outlook of its own: the reformers, arguing for a new, progressive political and economic model that would become an alternative to bureaucratic socialism; the anti-reformation bloc; and the Dubcek center group.

In the struggle over the question of changing the whole structure of power, the communication media became the main vehicle for advancing the cause of the reformers. The public airing of differences among Party factions and their infighting deepened the political crisis and weakened the Party's ability to lead and mobilize the people.

Further complicating the factional struggle among the several power elites was their interaction with the Soviet power elite. The turning point came with the publication of the Warsaw letter, signed by the Party leaders of the U.S.S.R., Poland, East Germany, Bulgaria, and Hungary, and addressed to the Czechoslovak Party Central Committee. The letter closed with an ultimatum to mobilize immediately and to fight decisively against all threats to the foundations of the Communist regime in Czechoslovakia. In a resolute reply, Czechoslovakia's leaders revealed the depth of their conflict with the Warsaw Five. The reply unified the Party and brought it great popular support. At the end of July came the fiasco of the meeting with the Soviet Party Presidium in Cierna, and three weeks later the military intervention by the Warsaw Five.

M relates how the Party placed itself in the forefront of the resistance and fought a despairing battle to preserve its political program of "socialism with a human face." But the conservative elements, whose fate seemed to be sealed just before the intervention, gradually regained control over the Party and provided the necessary ingredient which enabled the Soviet regime to accomplish successfully the main purpose of the military action.

Ivan Sviták, the author of *The Czechoslovak Experiment, 1968–1969,* identifies himself as a Marxist philosopher and socialist humanist. He was expelled from the Communist Party in 1964, and in the post-Dubcek period lost the position of a Fellow of the Institute of Philosophy of the Czechoslovak Academy of Science and a lecturer in modern philosophy at Charles University, Prague, as well as his Czechoslovak citizenship.

Sviták states that all the documents presented in his volume were written for the radically democratic Czech intelligentsia, especially the leftist democratic students. With the exception of the integrating Prologue, his essays have all been previously published, most in Czech newspapers during the Prague Spring. The book has an excellent selection of political cartoons, a welcome contrast to the earnest style of Sviták's materials.

Sviták argues that the Czechoslovak experiment was only the first stage of a democratic revolution, an overture to a radical structural change. In the first half of 1968, the goal of a complete transformation of the system, advocated by the radically democratic elements of the intelligentsia and the university students, did not have a wide backing among the main strata of the society. A broad-based revolutionary movement came into existence just before the Soviet military intervention. Sviták comments that the interpretation of the Chinese press, which called the experiment a factional struggle between revisionist power elites, is closer to the truth than the Western and Soviet interpretations.

Early in the Dubcek period, the author published his main theses supporting the movement for structural reform and civil rights. He justified his crusade against the bureaucratic dictatorship as a struggle against the perversion of the humanistic aims of Karl Marx's socialism. Marx, the messenger of freedom, provided no theoretical support for requiring the Communist Party to take the leading role. Sviták warned that "it must be clearly understood that we support the program of the new team, not their personalities, and that their maximum program is our minimum one" (p. 25); he also warned that "the slogan of 'democratization' is an improvisation born out of abnormal circumstances involving personality conflicts within the Politburo" (p. 26). He urged that no one who was a member of the National Assembly in the spring of 1968 should be elected and that the whole power structure should be liquidated and replaced by a more pluralistic political system, more sensitive to human needs and rights.

Sviták was aware of the danger arising from the endeavor to emancipate Czechoslovakia fully from the Soviet political model and to begin the process of transformation of the system as "an exceptionally important preview of social processes which would take place in the Soviet Union in the seventies" (p. 37). In an article written early in August, 1968, he analyzed the power rationale of Soviet intervention and concluded that "the overriding reasons for intervening in the internal affairs of Czechoslovakia rest on ideological and prestige considerations" (p. 132). The decisive reason was the well-founded fear of infecting other socialist countries with the ideas of democracy: "The Soviet leaders are willing to tolerate, in Eastern Europe, any kind of benevolent dictatorship but not political freedom based on respect of fundamental human rights" (p. 133). In his opinion, a more moderate course might have deferred but would not have prevented U.S.S.R.'s military action to restore "the elitist version of 'Panzer-Marxism'" (p. 212).

According to Sviták, the right course was the radicals' attempt to accelerate the internal democratization of the regime; there was no way of preventing the military intervention except by accepting the risk of war:

> The Czechoslovak experiment foundered on its own paradoxes—on the bizzare role assumed by the Communist Party, which tried to act in the guise of a democratic organization, on the naive belief of the power elite that its adversaries could be drowned peacefully in a wave of popular enthusiasm for democracy, and on the clash between the illusion of Marxist humanism and the reality of mechanized divisions. . . . The Czechoslovak experiment foundered on its effort to accomplish the impossible: the Europeanization of Russia and the humanization of Soviet Communism (pp. 214–215).

Will there again be opportunities to sponsor programs of political renewal in Czechoslovakia when the international climate is more favorable than in 1968? Sviták, like M, has not given up hope. By giving divergent viewpoints on the critical policy issues, the two volumes make a fascinating contribution to the understanding of the rise and fall of reform movements in Central and Eastern Europe.

JOSEPH HAJDA

Kansas State University

The Jews in Soviet Russia Since 1917. Edited by Lionel Kochan, with an introduction by Leonard Schapiro. (London: Oxford University Press, for the Institute of Jewish Affairs, 1970. Pp. 357. $7.95.)

Considering the large contribution scholars of Russian Jewish ancestry have made both to the social sciences and to East European area studies it is rather surprising that so little detailed research has been devoted to the Russian Jews as a social group. The student of Soviet nationalities can turn to an array of monographs dealing with almost every facet of contemporary or past development of the Ukrainians; materials on the Armenians and the Baltic nationalities are nearly as comprehensive. In contrast, treatment of the Jews of the Russian Empire and the Soviet Union has tended to take the form of polemics, descriptive history, or literary criticism.

The present volume is a partial remedy for this situation. While the symposium includes distinguished Americans like Maurice Friedberg, Professor of Slavic Languages at Indiana University, and William Korey of B'nai B'rith, the sponsor and a large majority of the contributors are British. In their treatment of "The Jewish Population: Demographic Trends and Occupational Patterns," the renowned economist Alec Nove and his Glaswegian colleague J. A. Newth provide the best behavioral analysis of any aspect of the Russian Jewish collectivity. Most of the articles, however, employ literary or descriptive approaches rather than behavioral science methodology. Consequently, these articles provide material for conceptual analysis rather than analysis itself. The service they render is, however, by no means negligible, for anyone (like the present reviewer) not of the Russian Jewish culture has had enormous difficulty even following its terminology and institutional complexities. Thus the articles on "Soviet Jewry," "Hebrew Literature in the U.S.S.R.," "Yiddish Literature in the U.S.S.R.," and "Jewish Religion in the Soviet Union," apart from the intrinsic interest of their subjects, will probably be most useful to political scientists as guides to using literature as source material.

Friedberg's two articles on the Jewish component in Soviet literature, on the other hand, provide more directly utilizable information. While the author approaches his subject in a literary manner, his findings can readily be related to the conceptual framework of social science. For example, Friedberg's analysis of successive periods of treatment of Jewish themes in Soviet literature suggests a general hypothesis on the relation of a highly centralized mobilization regime to an established minority culture. First Jewish characters appeared frequently and sympathetically, but were effectively drained of specifically Jewish tradition and cultural values; later (in the postwar Stalinist period) Jewish characters and themes nearly disappeared from Soviet literature, except in the veiled, pejorative form of "homeless cosmopolitans"; most recently (as in Andrei Siniavsky's works) Jews reappear in non-Jewish dissident literature as the symbol of rejection of the inhumane values of the regime.

Equally useful, from a different point of view, is "The Jews in Russia at the Outbreak of the Revolution." While S. Ettinger gives essentially a descriptive history, he also provides invaluable statistical data which up to now have been very difficult to obtain. Using primarily older Russian-language sources, Ettinger analyzes the natural increase, internal and external migration, and occupational distribution of Russian Jews in the two decades preceding the Revolution; while not resolving all uncertainties, his analysis enables one to derive concrete indicators of the transition from a traditional diaspora to one which was modernizing at a very rapid rate.

Nove's and Newth's conceptually sophisticated statistical treatment of the Soviet period is much more detailed than Ettinger's. Relying mainly on intensive analysis of Soviet data, the authors are able to show, with a high degree of probability, the dimensions of war losses, migration from the "Pale" to internal Russian areas, changes in age and sex ratios, and occupational distribution. They convincingly argue that there is little evidence that the declining *proportions* of Jews in general higher education and the skilled professions indicate discrimination, although the authors are careful to point out that this conclusion does not preclude the inference (supported by nonquantitative evidence in other articles) that Jewish quotas may exist in elite occupations and training institutes. Nove and Newth are even able to throw some light on the difficult question of intermarriage. Here, however, their study is handicapped by a common feature of nearly all of the articles in the symposium: Composition preceded publication by several years. As a result, Nove and Newth did not have access to the detailed, revealing Jewish demographic information which has appeared in N. V. Kurman and I. V. Lebidinskii, *Naselenie Bol'shogo Sotsialisticheskogo Goroda* (1968, on Kharkov) and A. I. Kholmogorov, *Internatsional'nye Cherty Sovetskikh Natsii* (1970, on Latvia).

The volume does contain a more up-to-date article, "After the Six-Day War," by Zev Katz. But, like preceding articles in this volume dealing with political decisions ("The Biro-Bidzhan Project" by C. Abramsky, "Soviet Jewry in the Second World War" by R. Ainsztein, and "Antisemitism in Soviet Russia" by B. D. Weinryb), Katz's treatment documents the Soviet regime's anti-Semitic and anti-Israel policies without really analyzing the reasons for such policies. The political scientist will find this lack of analysis in depth disappointing, but it is perhaps too much to expect a volume of this nature to present a particular theoretical interpretation which would presuppose, in effect, a general theory of the nature of Soviet politics. As the editor points out, "the intent of this book has been historical" (p. 337). In a few places, however, greater clarity even in the chronological presentation is needed. For example, Weinryb overlooks the very significant fact that the most atrocious act in the purge of Yiddish cultural figures, the official murder of Solomon Mikhoels, occured in early 1948 *before* Israel was established, and that, conversely, the anticosmopolitan campaign became vehement only after Andrei Zhdanov's death.

For the earlier period, Jacob Miller presents an excellent, though largely descriptive, survey of the evolution of Leninist-Stalinist theories about the Jews, and Korey provides a comprehensive discussion of the legal status of the Jews. Taken together with the other excellent articles of this volume, their work will enormously facilitate comparative studies on ethnic groups in the U.S.S.R.

JOHN A. ARMSTRONG
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Legislatures in Developmental Perspective. Ed. by Allan Kornberg and Lloyd D. Musolf. (Durham, North Carolina: Duke University Press, 1970. Pp. 590. $15.00.)

This is an impressive symposium which is mistitled, or at least the contributors do not agree on what the title signifies. For some of them the focus is on whatever relationship exists between legislatures and development (economic, political or both). For others the central issue seems to be whether legislatures can be perceived within a developmental, in this case powerless to powerful, continuum. Additionally, agreement is lacking on what legislatures are or do. On the one hand the contributors do not cavil at the assertion that it is "principally through the enactment of statutes that the modern [Western] legislature has derived its saliency" (p. 7). On the other hand Robert A. Packenham, who conducted a survey of functions performed by these institutions, concludes that "most [Western legislatures] do not allocate values, or at least do not have this as their principal function. Other functions . . . [presumably non-legislative, but ambiguity arises] seem to be more important" (p. 522).

We are thus confronted with a monumental muddle. Do legislatures legislate ("enact statutes," "allocate values")? A tentative "maybe" would seem to be the accepted answer. Should these "institutions"—bundles of interests, assemblies of representatives, etc.—legislate? Again, the response appears to be less than clear. It really depends upon what other goals are given precedence. Legislatures, especially if they are nonrepresentative by virtue of exclusiveness or a skewed electoral system, may impede rather than encourage political or economic development.

It is all very curious, and for me at least, highly instructive and encouraging. Let me explain. First and foremost, the symposium reflects a revival of serious scholarly research on legislatures. Much has been learned about the U.S. Congress and British House of Commons, but even about them, gaps in our understanding remain. Anyone who wishes to do further research in this area would do well to study Musolf and Riggs' chapter on "Comparative Legislative Studies and Services: Notes for a Program."

Kornberg and Musolf begin the volume with an introductory essay providing the background for the symposium and a précis of the contributors' major findings. Chapters 2 through 9 are essays on legislatures of selected countries (U.K., Canada, U.S.A., India, Chile, Lebanon, Kenya, and Philippines), representing a broad geographic mix as well as "developed" and "developing," and "parliamentary," "congressional," and "hybrid" models. Despite the undoubtedly prodigious efforts of the editors, there is an inevitable divergence of style and mode of analysis which can be readily explained by the disparity of basic knowledge concerning these institutions and their inmates. Compare, for example, Crick's mature reflections on the British Parliament with Agor's heavily footnoted protestations concerning the "highly developed" character of the Chilean Senate; or Kornberg's severe recriminations regarding the Canadian Parliament with Stauffer's comments concerning the Philippine Congress's vitality "in keeping democratic politics alive." Common criteria or expectations appear to be lacking, and a rather pernicious condescension is apparent: One can evidently criticize well-established legislatures and freely discuss their shortcomings, especially the perceived chasm between expectations and performance; but it is apparently not quite proper to find fault with similar institutions in less hospitable climes; nay, their performance must be extolled.

Fortunately, Packenham provides healthily abrasive constraints on the occasionally uncritical enthusiasm for legislatures and their possible roles in political and economic development. Basically his message is that our knowledge of legislatures within the context of specific domestic politics, and of the relationship between improving legislature services (research and administrative staff) and political development is as yet too slight to allow us to rush in with projects to strengthen legislative bodies:

> I have not written this paper to argue that under no circumstances should a program of technical assistance for legislative services be forthcoming. I have tried to suggest that these circumstances will rarely be found, and no such program should be undertaken until the kinds of questions raised here have been confronted and more satisfactorily answered (p. 582).

Such self-restraint is laudable, especially since championing such a program might well provide an A.I.D.-funded pot of gold at the end of the rainbow.

The symposium contains four analytic essays. Two deal with problems of professional staffs for legislatures in general and for committees, with the experience of the American Congress used as the principal data base. In the third essay John Grumm seeks to determine the "structural determinants of legislative output," with American state legislatures used as the data base. Finally, Malcolm Jewell assesses the utility of role analysis in probing "Attitudinal Determinants of Legislative Behavior," drawing also on American experience. Jewell's conclusion that role analysis "is a flexible technique, tested in previous research, and appropriate in comparative analysis" is a bit worrisome if for no other reason than that it implies an uncritical acceptance of particular research methodologies which may well be culture-bound.

Nonetheless, as noted at the outset, this symposium is a worthwhile contribution to the study of legislatures. Americanists will find it useful in reopening questions they might have considered as having been settled long ago. Comparativists will gain from it renewed challenges to answer questions that may be extraordinarily difficult to research. All this is to the good.

HANS H. BAERWALD
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Essays on Gandhian Politics; The Rowlatt Satyagraha of 1919. Edited by R. Kumar. (Oxford: The Clarendon Press, 1971. Pp. 347. $10.50.)

The Rowlatt Satyagraha marked an important stage in the growth of Indian nationalism: it propelled Gandhi into dominant national leadership and ushered in an era of mass politics. The Rowlatt Bills, so called after Sir Sidney Rowlatt who headed a government committee to investigate seditious activities in India, were drafted to confer upon the Government of India extraordinary powers to deal with "anarchical and revolutionary crime." Gandhi launched the first nationwide satyagraha movement in April 1919 with the immediate objective of forcing the withdrawal of the one enacted Bill and preventing the passage of the second by the Legislative Council. His long-range purpose was not only to mobilize popular support against British rule but also to create in Indians the self-reliance necessary for sustaining, as well as achieving, self-rule.

Nine of the ten essays published here were first presented at a symposium held at the Australian National University in 1966, and they reflect the expanding Australian interest in the study of modern India. From these essays emerges a picture of the political conditions in India during 1919. They explore such questions as the nature of the social and economic groups which responded to Gandhi's initiative, and the local discontents which Gandhi channeled into a movement of protest against British rule; and they assess the qualities of the primary and secondary leadership of the movement in different parts of the country.

The final essay by D. A. Low originally appeared in the *Journal of Asian Studies* (February 1966) and is addressed to the first non-cooperation movement which began a year after the Rowlatt Satyagraha. Dr. Low's study, together with others in this volume, adds weight to his opinion that the most fruitful advances toward an understanding of the processes by which India and Pakistan achieved their independence may well be made through the study of "particular episodes and particular themes" (p. 298). Dr. Low focuses upon government opposition to the nationalist movement and makes the point that with increasing interest in the movement itself there may be some danger that the changing strategies and tactics of the government response could be neglected.

This book is rich in descriptive historical detail of political action, both within the movement and between satyagrahis and government forces. The well documented essays on the action as it developed in the cities of Bombay (J. Masselos), Delhi (D. W. Ferrell) and Lahore (R. Kumar) and in the Central Provinces and

Berar (D. E. U. Baker) and Gujarat (K. L. Gillion) add substantially to an understanding of the complexities of the movement. Each also provides an analysis of the respective political and social backgrounds into which satyagraha was introduced.

In his introductory essay, Dr. Kumar observes that the Rowlatt Satyagraha illustrates Gandhi's ability to draw together individuals belonging to different castes, communities, and religions and that "what immediately strikes the attention of the social historian is the conspicuous absence of the language of class in the appeals which Gandhi issued in connection with the Satyagraha . . ." (pp. 12–13). Dr. Ferrell concludes that the "primary significance of the Rowlatt Satyagraha in Delhi was that, given an appropriate issue, it was possible to transform a heterogeneous political community into a homogeneous political society" (p. 235).

As Dr. Masselos notes, Gandhi considered the technique of satyagraha "not only the most effective method of protest against the Rowlatt Bills . . . but also it was the only method of preventing the outbreak of violence." Gandhi sought to provide an outlet for the passions of young Indians, passions which otherwise would, he wrote, manifest themselves in the form of " 'the powers of vengeance and the doctrine of the little Bengal Cult of Violence' " (p. 174). Gandhi was astonished at the intensity and extent of the movement's support. With the outbreak of violence in several places, Gandhi suspended the movement and went on a penitential fast. H. F. Owen critically evaluates Gandhi's leadership. He examines the new problems of organization which faced Gandhi in a nationwide campaign and examines the difficulties of exercising control and restraint over newly mobilized social groups in circumstances of agitational political activity. His essay contributes to an understanding of what was, in strictly Gandhian terms, a failure of satyagraha. Detailed critical evaluations of the secondary leadership are presented in the several essays on city and regional activities.

Two background essays treat respectively traditional influences upon Gandhi and his earlier development of satyagraha. A. L. Basham concludes that Gandhi was more successful than earlier reformers in giving non-Indian ideas a thoroughly Indian character by building on the foundation of his own tradition and relating new modes of approach to earlier doctrines and concepts. As Gandhi himself had pointed out, there were only two major planks in his platform which had no prototypes in India's past: the dignity of manual labor and the emancipation of women.

P. H. M. van den Dungen's penetrating essay challenges the view that the Rowlatt Satyagraha marked the turning point for Gandhi. He explains why Gandhi's position and leadership of this movement did not represent a sudden shift of loyalty from that reflected in his readiness, in 1918, to recruit for the army. Recruitment was part of his program to restore Indian manhood. Gandhi had, in fact, advocated the inauguration of satyagraha (to secure amendments in the Montagu-Chelmsford reforms) at the same time that he pressed for recruitment. Dr. van den Dungen goes to the heart of the matter in explaining central but often overlooked elements of satyagraha. He touches upon "the corollary of the ethics of nonembarrassment—helping the opponent in a time of trouble in order to secure a sympathetic response . . ." (p. 61). He analyzes Gandhi's development of satyagraha in South Africa and shows that the Gandhi of the years before 1905 "could not and would not have launched a movement like the Rowlatt Satyagraha of 1919," but that the Gandhi who emerged at the end of 1909 "was not only able to do so but was eager to use satyagraha and not least in the struggle for swaraj" (p. 59). Dr. van den Dungen's contribution serves to counter the argument (made here by Dr. Low) that Gandhi failed to appreciate the moderates. Closer alliances with the moderates might well have served immediate political purposes on several notable occasions. But Gandhi never lost sight of his ultimate objective which centered upon the further development of satyagraha, its use in situations of conflict, and the realization of the social, economic and psychological effects it promised for individuals throughout India. The Rowlatt Bills provided the occasion to replace violent means with satyagraha and to exercise what was, as van den Dungen establishes, an old policy. "Gandhi had been ready since at least 1909" (p. 63).

An American scholar (Dr. Bernard Kohn) has recently urged that social historians devise a more sophisticated model of the functioning of Indian society in the twentieth century and that they ask "what ties, what identifications, what symbols do [Indians] call upon and respond to in what circumstances?" This volume contributes to an understanding of the complex circumstances in which Indians found themselves in 1919 and illuminates the many ways in which Indians bound themselves to one another and gave expression to those ties. It suffers less than do most collections of essays from overlapping exposition and contradicting argument. The book is a valuable contribution not only to the continuing study of "Gandhian politics" but also to modern Indian political history.

JOAN V. BONDURANT
University of the Pacific

Elites in South Asia. Edited by Edmund Leach and S. N. Mukherjee. (Cambridge: Cambridge University Press, 1970. Pp. xiv, 266. $12.50.)

It has become a cliché in the reviewing of symposia that the quality of the contributions is "uneven." But what is a reviewer for a political science journal to say if practically none of the authors has shown the relevance of his research, however scholarly it may be, to the general topic designated by the title of the volume? If all were historians, one might simply acknowledge that our sister discipline has different criteria of significance and accept their wealth of factual data and cautious interpretations at face value. But the editors tell us that the seminar for which these papers were originally prepared was interdisciplinary, including a sociologist (Shils), an economist (Bagchi), and three political scientists (Bernstorff, Dalton, and Gray) as well. In these circumstances it seems insufficient for Leach and Mukherjee simply to enter the modest disclaimer that "the level of communication was not always very high" and that "for most of our authors, theory has been of minor relevance." Was it not their obligation as convenors to stipulate the terms of reference, such as the definition of elite, for the participants so as to ensure some degree of comparability of results? True, they have struggled *post hoc* in the introduction to set the book's theme into the context of European political sociology: Pareto, Mosca, and Marx. They find as a "common parameter" that all the papers but Spear's are concerned with the British influence on the Indian elite. With the dubious exception, however, of C. Wright Mills, they have ignored the whole North American controversy represented by Lasswell, Dahl, Bachrach, and Bottomore about the definition of elites and the measurement of power. Certainly it no longer suffices to "let the contributors decide for themselves what they mean" by the basic conceptual tool of the book. They conclude that "for most [of them] . . . the word elite implies no more than 'the men at the top'" (p. x) when, by their own admission, what most of the groups studied have in common is precisely that they were *not* at the top: they were colonial subjects! At most, they qualify as subelites or as a potential ruling class.

These conceptual criticisms aside, what raw materials do the eleven authors provide the student of elites for eventual generalization? The editors have remarked pessimistically that their contributors have not shown that anything is generally true about elites in South Asia but that they have demonstrated quite a number of things which are *not* true. The chapters can be classified, according to the source of power they deal with, into four categories: (1) the traditional landed aristocracy (Percival Spear on "The Mughal Mansabdari System," Eric Stokes's "Traditional Elites in the Great Rebellion of 1857," and Hugh Gray's "The Landed Gentry of Telengana"); (2) the modern commercial class (Ashin Das Gupta's "The Merchants of Surat," S. N. Mukherjee's own "Class, Caste and Politics in Calcutta, 1815–1838," Christine Dobbin's "Competing Elites in Bombay City Politics in the Mid-Nineteenth Century," and Amiya Kumar Bagchi's "European and Indian Entrepreneurship in India 1900–30"); (3) the modern intellectuals (Gordon Johnson's "Chitpavan Brahmins and Politics in Western India in the Late Nineteenth and Early Twentieth Centuries," Dennis Dalton's "M. N. Roy and Radical Humanism," and Edward Shils's "The Academic Profession in India"); and (4) the postindependence political class (represented by Dagmar Bernstorff's "Candidates for the 1967 General Elections in Hyderabad, Andhra Pradesh," and by Gray's paper on Telengana cited above.)

Only Spear and Bernstorff, two of the three authors dealing with pre- and postcolonial elites, explicitly raise broader analytical questions about an elite: its composition (caste and religious), its functional skills, its recruitment (including adaptability and circulation), its method of self-management (implying its cohesiveness) and the reasons for its decline or collapse. The chief impression one gets of the composition of these groups, whether truly elite or not, is the extent to which (at least among Hindus) high ritual caste status initially predetermined the chances of an individual's entering the new modern classes of India despite the introduction of achievement criteria. Thus Mukherjee, after denying that caste had any part in the selection of the *bhadralok* (gentlemen) of Bengal, admits that the top three castes (*Brahmins*, *Baidyas*, and *Kayasthas*) formed the bulk of them because they traditionally had a monopoly of the literary and administrative skills. Unlike the Muslims, they were ready to adapt them to modern requirements. Once a greater rate of circulation of elites did set in, whole subcastes (*jati*), upwardly mobile in wealth through collaboration with the colonial rulers, used the new resource to raise their supposedly immutable religious status by the now familiar process of Sanskritization. Gray's *Shudra* "gentry," for instance, claimed *Kshatriya* (warrior) status.

The much maligned commercial elites have been rather neglected by political historians and analysts of India, more recently, perhaps, because of a Marxist bias against them. The four articles I have categorized under this heading are a healthy corrective. They make one wonder if "socialism" is actually a cloak for *Brahmin* bureaucrats and communists to dispossess upstart *Vaishya* businessmen. If so, this process may provide a rationale for including in the book Dalton's Radical Humanist, Roy, as well as Shils's academics. While "elitist in temper," both would seem otherwise misplaced in a work on elites or even subelites. In Bombay (Dobbin) more than Calcutta (Mukherjee) it appears that the first intellectuals sprang in part from that very commercial class which their ideological heirs now seek to destroy.

Beyond the foregoing classification of Indian elites, one can ask about their cohesiveness. Presumably a high degree of organization must be characteristic of a group which aspires to elite membership. But these case studies picture intense competition within as well as between communities: orthodox vs. modernizers, commercial land buyers vs. traditional landowners, colonial collaborators vs. rebels. It was only a part of each subcaste, religious sect, or functional group which rose in power and status during the modern era.

Finally, Spear has asked of his materials a question which few of his coauthors have tried to answer: How does an elite retain what power it has, or how does it decline? Each group discussed in this volume is portrayed either during its rise or at its height. Yet we know from beyond the confines of the brief periods surveyed that all except the two postindependence cases were doomed subsequently to fall from political elitehood: Spear's *mansabdars,* Stokes's *talukdars,* Das Gupta's Surat merchants, Dobbin's Bombay intelligentsia, certainly Mukherjee's and Dalton's Bengali *bhadralok,* Johnson's Chitpavans who "abdicated from politics" into the professions, and Shils's academics. Perhaps what is needed next in the study of elites is an international seminar to create a sequel to this one, investigating how elites decline and others succeed them in South Asia, a subject difficult for upwardly mobile American scholars to comprehend.

THEODORE P. WRIGHT, JR.
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Venceremos Brigade: Young Americans Sharing the Life and Work of Revolutionary Cuba. By The Venceremos Brigade, edited by Sandra Levinson and Carol Brightman. (New York: Simon and Schuster, 1971. Pp. 406. $3.95.)

¿Does Fidel Eat More Than Your Father?: Conversations in Cuba. By Barry Reckord. (New York: Praeger Publishers, 1971. Pp. 191. $5.95.)

A number of Cubans ought to share in the royalties of these two books for having contributed at least as much to the writing as the authors themselves. Both the participants in the Venceremos Brigades and the black Jamaican playwright Barry Reckord looked to Cuba for inspiration. The Venceremos members considered the Cuban political and social structures and the development of "new Socialist man" as goals toward which the revolutionary movement in America should work. Reckord sought to document social, economic and spiritual progress in Cuba—and the price of that progress—measuring conditions in Cuba against those in his own Caribbean homeland.

In their relationship to the material, however, the authors differ considerably. The members of the Brigades considered Cuba not so much a model to be imitated, but rather a revolutionary setting in which they could test their mettle, their capacity to function collectively, and their commitment to work toward radical change in America. Their objective was to participate as fully as possible in the Cuban Revolution by cutting sugar cane and by trying to live and work as revolutionary men and women. Barry Reckord, in contrast, displayed no such wish to participate in the Revolution. As an intellectual alienated from his own country—he lives in London, not in Jamaica—he undoubtedly would have been unwilling to live as a Cuban. Nonetheless, he did attempt to provide a format through which citizens of Cuba could speak for themselves.

Venceremos Brigade is written by members of two of the four groups of Americans who have gone to Cuba to cut sugar cane or to work in agriculture. It is edited with a long introduction by Sandra Levinson and Carol Brightman, both of whom have first-hand knowledge of Cuba (Brightman participated in the Brigade).

The book is divided into eight sections, discussing preparations, work, leadership and discipline, the third world, women, revolutionary culture, internationalism, and "the aftermath." Materials include excerpts from the diaries, letters, speeches, essays, interviews, poems and pictures of the "book collective"; thirty-five writers, ten photographers, and an unspecified number of Cubans (including Fidel Castro) contributed.

The editors' and writers' hope was that out of the Cuban experience would emerge a greater sense of self, an appreciation for the need to work together, and a stronger commitment to and respect for the potential of the American people. The editors' introduction explains the formulation and organization of the Brigades, and provides a useful and thoughtful analysis of the Brigades' importance to the American left. "What happened to the *brigadistas* in Cuba . . . gives us more insight into the strengths and weaknesses of our movement than any single activity in the United States" (p. 16). The primary objective in putting together the book was to share this insight rather than to analyze Cuba. In fact, the sections which treat conditions and problems in Cuba are unconvincing, since the Brigade members became familiar with only a small and unrepresentative sector of Cuban life.

While the Americans expressed joy and appreciation for the chance to live and work in Cuba, the Cuban experience challenged their perception of themselves as revolutionaries. The dominant theme in the book, and the subject of the perceptive analysis in the Introduction, is the group's failure to act collectively. Competitiveness, male chauvinism, the inability to deal with race problems, and the tendency to think of revolution in terms of personal acts ("an extension of good old frontier individualism" [p. 30]) frustrated the well-intentioned Americans and confused the Cubans. The Cubans tried to help the Americans to deal with these issues, but they could neither fully understand nor cope with the group's divisions.

The improbable title—*¿Does Fidel Eat More than Your Father?*—masks a serious book which explores many dimensions of Cuban life. We soon find out from a group of Cuban children that Fidel does not necessarily eat more than father, or that if he does it is for good reason, or maybe in fact his problem is that he eats too much. There are, after all, many perspectives on the Revolution, and there are both willing and unwilling revolutionaries. After an initial chapter devoted to his overall impressions, Reckord has organized his conversations around three themes: liberty, equality, and economic development. Considering these as the objectives of any revolutionary process, he seeks to measure the Cuban Revolution against them. A final chapter on Cuba and the United States contains a few concluding remarks by the author and long sections of a speech by Fidel.

In Reckord's book, Cuba is represented to the readers through the words of as wide a variety of *Habaneros* as he could find to interview: blacks and whites, middle class and lower class, intellectuals and bureaucrats, men and women, old people and youth. The urban scene is thoroughly represented, but impressions of rural Cuba, which has been the major focus of revolutionary efforts, are missing. Cuban opinions are recorded, presumably verbatim, occasionally interrupted and given direction by the author's observations and his interminable probing questions.

Reckord's tone throughout the book, but especially in the early chapters, is cynical. He dislikes much of what he sees in Cuba, reports a fair number of horror stories, and quotes extensively from people few of us would consider to be pleasant as neighbors. Pessimistically he considers most Cubans to be "consumers" of the revolution, rather than its creators. The commitment of Cubans to voluntary work in agriculture and to community service disturbs more than it impresses the author, and it leads him to conclude that "there is not much private virtue, only collective virtue" in Cuba.

Nevertheless, on the all-important measures of liberty and equality, Reckord judges that the Cuban revolution comes out quite well. Traditional civil liberties, to be sure, he finds lacking, but he is impressed by the degree to which young Cubans are informed, involved, and enthusiastic particpants in national development. In his own country the same young people would be ignorant, poor, and apathetic. In the section on equality, he effectively describes revolutionary shortcomings, such as lingering personal racism despite the elimination of institutional racism (being black, Reckord is especially sensitive to the situation of black Cubans), persistent bureaucratic privilege, and the structural inadequacy of increased worker participation. Nonetheless, Cuban egalitarianism excites his imagination and admiration: Socialism, he says, does not result in "dreary equality because when the dirty work is shared all around, then everybody will have the time and the mind to be creative."

In his treatment of the theme of economic development, the author's sympathies seem to be with Fidel rather than with the foreign experts whom Reckord describes as continually telling the Cuban leader that his priorities are wrong because they are expensive. Reckord dramatizes the dilemma of socialist economics with an account of British experts and Cuban cattle. Fidel, unable to trust foreign advice, makes his own decisions by instinct, and Reckord tells us why this is so.

While neither of these two books represents conventional academic scholarship, both studies are interesting and useful. Just as it is valuable to study Cuban society itself because it is perhaps the most exciting and controversial socio-political experiment in the Western Hemisphere, so too is it important to understand the direct impact of the Cuban experience on the youth of our own country and on representatives of the Third World.

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Marxist Theory and Indian Communism. By Charles P. Loomis and Joan Huber Rytina. (East Lansing: Michigan State University Press, 1970. Pp. 148. $7.00.)

This book discusses Marx's contribution to social theory and its political application in India in terms of conceptual categories for structural-functional analysis. These theoretical categories, derived from such sociologists as Tönnies, Sorokin, and Parsons, formed the basis of the "Processually Articulated System" (PAS) model first put forth by Professor Loomis in *Social Systems: Essays on Persistence and Change* (1961) as an aid to comparative analysis of social systems.

In the present work the authors claim that the PAS model facilitates a systematic analysis of social theories, such as Marxism, as well as an understanding of the problems encountered by Indian communists in adapting Marxism to local conditions. There is some justification in attempting to explain the social-system model of the Marxists in terms of the functionalist approach, since both models view economic development as a major concomitant of modernization. This reviewer doubts, however, whether the functionalist model provides "a set of neutral categories" (p. 5) by which to examine other models, as the authors claim. The Marxist and functionalist approaches—the arguments of Dahrendorf, Van den Berghe and others to the contrary—appear to be contrasting ways of perceiving political reality. The functionalists stress accommodation or consensus, put forth the concept of a political system in equilibrium as the ideal type, and view "political development" as the gradual elimination of conflict; whereas the Marxists emphasize conflict as the essence of politics and the source of "political change." Comparative politics "revisionists" are currently putting forth social-systems models more closely approximating the latter view than the former. In the present work, the theoretical bias of the authors is clearly evident in their evaluation of Marxian social theory.

Marx is depicted as primarily a sociologist and only secondarily an economist in the sense that he attacked the classical economists for ignoring social realities but accepted without question most of their theories (pp. 29–30). Despite the solid, empirical basis of Marx's scholarship, his predictions have gone astray, the authors contend, because of his faulty epistemological assumptions which posit a uniformity between logic and empirical reality and assert a necessary correspondence between dialectical categories, (such as thesis and antithesis) on the one hand and historical stages (such as feudalism and capitalism) on the other. This claim to universal validity on behalf of the dialectic has contributed to the fusion of theory and practice in Marxist thought. As a result, it is argued, dialectical materialism is similar to pragmatism in that both claim the criterion of truth to be how things work out in practice, and "anyone with enough power can demonstrate the truth" of Marxist claims (p. 16).

The Marxist treatment of social processes is discussed under such structural-functional rubrics as communication, boundary maintenance, systemic linkage, institutionalization, socialization, and social control. In the process, familiar Marxian ideas are converted to neologisms. Thus, class-consciousness becomes "boundary maintenance mechanisms" (p. 70), and alliance with the national bourgeoisie, an instance of "systems linkage" (p. 71). Although an interesting intellectual exercise, this procedure does not appear to further our knowledge of Marxist theory or to help explain the problems encountered in attempting to apply this theory to Indian conditions. The crucial question in both cases is whether the PAS Model provides categories which are truly analytical in nature, rather than purely descriptive: Does it advance our understanding of empirical reality? On the evidence solely of the present study, the answer would have to be no.

On the crucial question of India's political future, this work does not shed any new light. Both Marxists and structural-functionalists have been able to identify certain preconditions for political mobilization, but have been less successful in predicting the various political forms that mobilization will take. The discussion of Indian Marxism, moreover, is seriously flawed by a lack of familiarity with the subject at hand. For data on both communist developments and socioeconomic conditions in India the authors rely heavily on two secondary sources—Overstreet and Windmiller's *Communism in India,* published more than a decade ago, and Gunnar Myrdal's *Asian Drama: An Inquiry into the Poverty of Nations.* As a result, the treatment of Marxist thought in India is disappointing. Much relevant literature by Indian Marxists has been overlooked. Discussions of important issues are sketchy and not wholly accurate in all cases. For example, we are told that "Indian Marxists evaluated the potentials of various classes somewhat differently than did the orthodox Marxists" (p. 41), whereas, in fact, Indian communists are as divided on this issue as their counterparts elsewhere. The question was first debated at the Second Comintern Congress in 1920 and is today a bone of contention between Moscow and Peking.

In a later discussion of the special problems of communication for Marxists in multi-lingual India, the authors cite the controversy over the term "decolonization" at the Sixth Comintern Congress in 1928 as a case in point. The various interpretations attributed to the concept in the course of the debate, however, had nothing to do with the language problem in India, as the authors suggest (pp. 68–69), but were related to the internal power struggle within the Soviet party. Finally the continuing influence of Gandhi's ideas, especially on nonviolence, is viewed as a major obstacle to the spread of communism in India (p. 51). Gandhi's views are honored more often in the breach than in practice today. Certainly faith in nonviolence has not been noticeably evident. The real difficulties faced by Indian communists have more to do with: their lack of ideological and organizational unity, the smallness of India's labor force relative to its immense population, India's steady if not spectacular economic progress, the hostility of Communist China, and the existence of both a competent administrative apparatus and a party in power which is able to command the loyalty of wide segments of Indian society.

This volume contains two appendices. Appendix B is a brief summary of the PAS Model. Appendix A consists of two parts: the first, by Mohit Sen, a member of the National Council, Communist Party of India, is an attempt to defend dialectical materialism against the attacks of the authors; in the second, Dr. Gautam Mathur, Chairman of the Department of Economics, Osmania University, Hyderabad, comments on the inapplicability of Marxian analysis to modern economics. This reduces the major portion of the book to slightly more than 100 pages—which, at $7.00, is a rather expensive publication even by today's inflationary standards. Regardless of their theoretical disposition, students of political sociology and political theory, especially of Marxist thought, may well wish to acquaint themselves with this work. Students of Indian communism, however, would find the effort less rewarding.

JOHN PATRICK HAITHCOX

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Bolivia: The Uncompleted Revolution. By James M. Malloy. (Pittsburgh: University of Pittsburgh Press, 1970. Pp. 396. $11.95.)

This book on the Bolivian revolution is ideal in many ways. Its great length enables the author to spend about half the book on the events leading to the 1952 revolution, and the remainder on the governing accomplishments and political strategies of the *MNR* (*National Revolutionary Movement*) party to its deposition in 1964. Both parts of the book are equally good. Professor Malloy eschews chronological history for sharp analysis, continually asks the right questions, has excellent perspective with depth studies where needed, and never generalizes beyond his research findings.

The thoroughness of the research sets the tone for the whole book. The work is basically Professor Malloy's doctoral dissertation. To write it he spent fourteen months in Bolivia poring over books, articles, and newspapers on the revolution, and supplemented that with depth interviews from a number of prominent and secondary leaders. The writings on the Bolivian revolution by Robert Alexander, Richard Patch, Herbert Klein, and others are also utilized and interpreted, and the author has even gone to the extent of scrutinizing many of Professor Alexander's written interviews of Bolivian party leaders and labor officials.

Objectivity is another commendable attribute of the book. Professor Malloy is basically interested in understanding the Bolivian revolution and thus avoids praising or deploring it. His detachment thus enables him, for example, to analyze the *MNR* party as an actual political power structure instead of whitewashing it, as others have done. He is capable of dissecting the formation and dissolution of hard-boiled coalitions without being caustic and cynical. The reasons for actions are what fascinates the author, and he avoids moral judgments.

One can always ask whether a book of this type will have any value to those other than Latin American specialists. In this case the response would definitely be affirmative. Professor Malloy in the first part of the book not only tests but also refines certain theories of revolution, and in the second part he evaluates many of the *MNR* revolutionary activities within the framework of some of the general literature on political development, mobilization, and economic choice priorities. Fortunately, the author never distorts his research findings to prove a theory. Instead, he has given us a case study in depth about the implications of a revolution in an extremely underdeveloped country. The Bolivian experience is in many ways unique, but the author argues that revolutionary leaders in other primitive countries will be faced with similar kinds of problems. Herein lies the value of the book for comparative politics.

In the first part of the book Professor Malloy utilizes various ideas from Chalmers Johnson's *Revolutionary Change* to sharpen his evaluation of the causes of the Bolivian revolution. The ideas are taken not for the purpose of constructing a tightly operationalized research model. Instead the author feels that the ideas enable him to place the causes of the Bolivian revolution in much better perspective. The best example is Malloy's insistence that it was the economic stagnation of the whole tin economy by the late 1920's, and not the Chaco war, that led to multiple dysfunctions in Bolivia. One might take issue with this singular emphasis, but the author describes how the stagnation in tin led to elite infighting, the scarcity in prestigious jobs for the middle sectors dependent upon tin, worker discontent, and the economic deterioration of Bolivia. Malloy goes on to show how Bolivia was doubly hit by the effects of the depression and the fighting of the Chaco war in the 1930's. He analyzes these as two huge accelerators which aggravated the discontent, leading to the growth of many reform parties, the discontent among younger military officers, the outpouring of intellectual criticism, etc. Professor Malloy argues that the tin miners were probably the most embittered group during the 1936–1952 period since unsympathetic regimes deprived them of gains they had made under Busch and Villarroel. The author is certainly not the first one to handle deprivation theory, but he utilizes it in an insightful way.

Some criticisms of the first part can be made. The author argues convincingly that the tin magnates of Patiño, Hochschild, and Aramayo were perceived as La Rosca, a hated oligarchy, and operated as an associational interest group on weak governments. But he does not specifically describe their ways of achieving influence. Also, in getting at the causes of the revolution, Malloy does not refer to Crane Brinton's very germane book, *Anatomy of A Revolution*, which deals with divided elites, the desertion of the intellectuals, economic incompetence, the misuse of force, etc. This is particularly unusual since Eldon Lanning thought Brinton stimulating enough to rigorously test his ideas in his dissertation *The Bolivian Revolution of 1952 and the Cuban Revolution of 1959: Case Studies of a Theory of Revolution* (unpublished doctoral dissertation, University of Virginia, 1965).

The second half of the book, covering Bolivia after 1952, carefully develops two interesting themes. The first is that Paz Estenssoro, Hernán Siles Zuazo, and other important *MNR* leaders were reluctant revolutionaries, who believed that the revolution should be controlled from above, and who preferred the creation of economic capital over the immediate distribution of the country's wealth to supporting revolutionary groups. Malloy develops this theme by recounting how the leaders continually conspired with disaffected military officers to come into power, and by showing how the leaders were not only somewhat conservative on the issue of nationalization of the mines and land reform, but also later moved into policies of stabilization and technocracy.

The second theme is that the *MNR* leaders, no matter how hard they tried, never really controlled factions within their own party, let alone their supporters such as the miners, the rural Indians, and regional bosses. Professor Malloy does his best writing on this theme. His analysis of the *MNR* right, the pragmatic nationalist center, and the left factions explains why they were so contentious. His excellent chapter on the rural Indians describes the uncontrolled land seizures of 1952 and 1953 and the great variety of decentralized rural sindicatos accountable only to local and regional bosses. His discussion of the miners' interests also emphasizes autonomous power and immediate consumption goals. Finally, the author's superb evaluation of the fragile political coalitions each president had to put together from 1952 to 1964 leaves one with the feeling that governing Bolivia was an almost impossible task. Events in Bolivia since 1964 have confirmed the author's conceptualization of the political system. The revolution has not been set back; groups gaining from the revolution are still autonomous, fragmented, and decentralized; and the search for national authority is still nebulous at best.

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The Challenge of Nationhood: A Collection of Speeches and Writings. By Tom Mboya. (New York: Praeger Publishers, 1970. Pp. 278. $7.50.)

Tom Mboya was one of the chief architects of Kenya's independence, its constitution, party apparatus, union organization and planning machinery. In the brief span of seventeen years Mboya rose to leadership of the trade union, nationalist, and pan-African movements and held a succession of key ministerial positions including Labor, Justice and Constitutional Affairs, and Economic Planning and Development. He was also chairman of the Economic Commission for Africa and the organizer of massive student airlifts for studies overseas.

This book was published a year after Mboya's tragic assassination. It is not a hastily mobilized effort to commemorate the passing of another fallen leader but a book Mboya had nearly finished before his death. His earlier book chronicled the justifications and actions leading to independence and the responsibilities this new status entailed. This posthumous work goes much farther, evaluating knowledge gleaned from an additional half decade of development experience.

Mboya's speeches and articles form a compendium of some of the most cogent arguments for nationalism, independence, planning, and the contributions of the new states and of arguments against tribalism, big-power competition, and foreign aid-trade practices. It is well enough integrated to be useful in courses on economic and political development as well as African politics.

The opening chapter of the book is an attempt to catalog the principal problems threatening the self-confidence and self-realization of the new African nations. Mboya deals summarily with a host of issues ranging from national interests, inherited institutions, rural development and one-party states, to women, youth, labor, the press, and the civil service. There are three following sections: "Kenya," "Pan-Africanism," and "Africa and the World."

Reading the selections, one is struck by their refreshing boldness and candor. In his addresses to newsmen, European farmers, students, professional societies, and to his parliamentary colleagues, Mboya moves directly to the crucial issues, squarely meets contradictions and resistance, and deftly selects the most alarming, though reliable, statistics to make his point. A typical speech, for example, indicates that "industrial output for all of Africa except South Africa is less than that for Sweden alone and contributes only 14 percent of our national income . . ." (p. 197). At a Canadian conference, Mboya, in presenting evidence of the nutrition gap in developing countries, concludes that "these estimates imply that both today and in 1975, the cost of a food aid program to close the gap between social need and effective demand . . . is about 6.5 billion dollars or eight times the size of the present effort" (p. 257). In discussions of trade preferences or commodity prices, his grasp of technicalities and fundamentals is extraordinary for one whose only formal training beyond secondary school was a year or so at Ruskin studying trade unionism.

His intellectual sensitivity in other areas is keen as well. He explicitly recognizes tax policies as a tangible expression of political value allocation. He identifies political rhetoric and slogan-making as tactics appropriate to early national consolidation but of no utility in later transitional politics. On the subject of contemporary Afro-Americans who romanticize Africa, Mboya asserts,

> . . . we live in mud and wattle huts and buy cheap Hong Kong fabrics not because it is a part of our culture, but because these are conditions imposed upon us by poverty and by limitations in technical, educational and other resources. White people often confused the symbols of our poverty with our culture. I would hope that black people would not make the same error (p. 228).

Mboya's definitions of the challenges of nationhood are terse and numerous, but he also includes a wealth of useful technical data and empirical observations, especially about such topics as foreign aid, planning, and economic development. His analyses do not comprise a development theory, though many of his statements (for example, his arguments for regional economies of scale) are useful adjuncts to such theories. Many points in the book are made didactically and are severely, though skillfully, simplified. Mboya was a man of practical affairs who saw it as his responsibility to chastise, encourage, and lecture those whose understanding and power were required to achieve the objectives he envisioned. The simplifications of such complex matters as Marxism, trade preferences, or African socialism are intended to educate the ignorant and misinformed among his own people as well as those outside Africa.

Some of Mboya's operating assumptions as reflected in this book deserve criticism. Two problems stand out. The first is his obsession with economism. In his discussion of African Socialism he cautions against excessive constitutional manipulation lest it put off foreign investors. In dealing with modern institutions and social change he tends to interpret them in the narrow terms of economic growth at the expense of political, administrative, and social considerations. Mboya is an excessively enthusiastic advocate of neo-Keynsian economics, the work-ethic, and the grand promises of technological shortcuts. Yet his very enthsuiasm for these orientations in no small way explains his own capacity to escape the crises of confidence which he saw all about him.

The second criticism one might level at Mr. Mboya's position is his uncritical acceptance of the colonial and British simplification of the politics-administration dichotomy. Kenyan administrators often contend that politicians do not acknowledge that they have a vital role in the political process. Mboya's assumptions about administrative neutrality, delegation, and responsibility are not supported by what we know about the dynamics of controlled political change. This leads Mboya into a distorted concept of party organization as well, despite his own record as a successful party tactician. His practical resolution of the tensions between the civil servants and politicians was to make the party a personal instrument. As a consequence there were massive seepages of power to the executive and hence to the administration. These seepages eroded various legitimizing electoral, representative, and administrative principles; they left the party an intermittent and ineffective spokesman for diverse social interests. Responsibility for policy has subsequently become so ambiguous as to ensure confusion; and tribal competition for party positions inevitably found an area for expression in the ministerial apparatus instead.

The Challenge of Nationhood is an excellent introduction to the present crises of nation building. In addition it is a useful memorial to a man whom time may identify as one of the most able and effective political leaders in modern African history.

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Résultats des élections belges entre 1847 et 1914. Bibliothèque de l'Institut Belge de Science Politique. New Series, no. 8. By W. Moine. (Brussels: The Institut, 1970. Pp. 200. $3.50, paper.)

Of the three states—England, Sweden, and Belgium—that have passed through what Stein Rokkan has called the "Western development toward equalitarian electoral democracy" in anything like a regular sequence, Belgium is the one whose development is by far the least well known. Yet Belgium is of interest for its own political history and for comparative studies: 140 years of electoral competition and parliamentary life unbroken except for the two German occupations; for half a century a two-party system less blurred than that of the British model; secrecy of the ballot introduced in the days of the taxpayers' suffrage; two decades of an intriguing system of plural suffrage, with a fourfold weighting system to temper the introduction of universal manhood suffrage; proportional representation since 1900; and for 70 years, until the 1965 elections, a basically three-party system, which makes election returns easier to handle than those of almost any other multiparty system.

The data on national elections since universal equal manhood suffrage in 1919 had been assembled, in publications to which I shall return. But there was almost nothing available by way of collected data for the elections between 1831 and 1892, and only bulletins of the Ministry of the Interior for the elections between 1894 and 1914. Now W. Moine (not M. Moyne, as cover and title-page of this errata-ridden book have it) gives us the results for the House of Representatives from 1847 to 1914, at the level of the electoral *arrondissement*, of which there were 41 until 1900, and 30 thereafter, with recapitulations at the level of the nine provinces and the kingdom as a whole. This covers all the elections before universal equal manhood suffrage except for the years 1831–1845, which it is reasonable to omit because of the uncertainty of assignment of candidates to parties in this period of Catholic-Liberal "unionism." Even for the later years, the author had to resolve many knotty problems of party assignment for candidates running alone and without party labels.

There is no substitute for the patient, detailed, and seldom adequately recognized work of the specialist, steeped in the problems and sources of time and place, who brings together the single-nation electoral data that one needs for further empirical and theoretical work. Moine worked almost alone, under the aegis of the eminent Professor De Meyer and the Center for Political Studies in the Flemish section of the University of Louvain. Not surprisingly, therefore, his work offers much less detail, fewer recapitulations, and less explanation, in a form much harder on the eyes, than the impressive *Atlas des élections belges, 1919–1954,* by De Smet, Evalenko, and Fraeys. That work, which is not an "atlas," gave the House results by the 211 *cantons*, with recapitulations at *arrondissement*, provincial, and national levels, and by urban agglomerations and by language areas, along with succinct summaries of the issues of each election.

Moine's few explanatory pages are helpful for details which the election returns fail to disclose, such as the lack of contests in some constituencies, the results of runoff elections before 1900, and the distribution of seats between the two parties in a number of Liberal-Socialist joint slates after 1894. The author does not attempt to explain the changes in the system of representation which give Belgian political history much of its significance and color. Occasionally he offers a few bland words that appear to take for granted the outcomes of controversies that raged (and in some cases still rage) in Western polities. For example, he remarks (page 170) that it was "normal" that proportional representation be adopted in 1899. Why it was "normal" in Belgium, when it was excluded in France and Britain, both of which greatly influenced Belgian politics in other ways, is left without explanation. The decision of the Catholic Party leaders, with their absolute majority in both Houses of Parliament from 1884 to 1914, was a vital one, "normal" or not. It saved the Liberal Party from the near-elimination from Parliament which was to overtake British Liberals under a majority system. And it prevented a full confrontation between a predominantly Catholic Flanders and a predominantly Socialist industrial Wallonia.

Those who wish to study Belgian developments will find published data now, not only in Moine and in the De Smet, Evalenko, and Fraeys *Atlas*, but also in *Les Elections belges*, a volume by De Smet and Evalenko comparing the results of the 1950 and 1954 elections with social structure, trade union preferences, and very shaky figures on religious practice; in election results and analyses in a volume of the Belgian Institute of Political Science for the 1958 elections, and special issues of its journal *Res Publica* for the 1961, 1965, and 1968 elections, as well as current reporting and analyses of primaries and both municipal and national elections in the *Courriers Hebdomadaires* of the Centre de Recherche et d'Information Socio-Politiques (CRISP) since its foundation in 1958. The electoral laws are described by John Gilissen in *Le Régime représentatif en Belgique depuis 1790*. Specialists will want to see the criticisms of Gilissen in a review by H. T. Deschamps in the *Revue belge de Philologie et d'Histoire*, 1959, and those of *Les élections belges* by Jean Stengers, with reply by Evalenko, in the *Revue de l'Université de Bruxelles*, 1958.

Wilfried Dewachter analyzes other steps in the electoral decision-making process: the choice of candidates, the issues, motivations and types of voting behavior, including preference votes, and consequences of the Belgian PR system in *De Wetgevende verkiezingen als proces van machtsverwerving in het belgisch politiek bestel* and in an article (in English) in *Res Publica*, no. 3 (1967). Frédéric Debuyst offers information and insight on the primary elections of the three major parties in *La Fonction parlementaire en Belgique: mécanismes d'accès et images*. There is an extensive bibliography of Belgian elections since 1831 by H. Lhoest-Offermann in *Res Publica*, no. 1 (1966). Dewachter has published an *Atlas* of the 1968 election results. But public opinion polls are just beginning to touch on party preferences.

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China: The Revolution Continued. By Jan Myrdal and Gun Kessle. (New York, Pantheon Books, 1970. Pp. 201, $5.95)

Ideology and Practice: Evolution of Chinese Communism. By James Chieh Hsiung. (New York, Praeger, 1970. Pp. 300, $4.50 paper)

These two works are welcome additions to the growing Western literature on contemporary China. Though drastically different in approach and coverage, both deal with the cultural revolution. The Myrdal book is an eyewitness account of the human drama of that revolution in a northwestern Chinese village. Hsiung's work interprets the outbreak of the revolution as a culmination of "the rising tension between ideology and practice" since 1949.

After eight years of absence, the Myrdals returned to Liu Ling, the village they had visited in 1962 and described in detail in their much acclaimed *Report From A Chinese Village*. The result is a sequel to their earlier work and the first comprehensive report by foreigners on the cultural revolution at the grass-roots level. Like their earlier work, this one provides a wealth of information about rural China which otherwise would be hard to come by, and it sheds light on the recent changes in Chinese villages. It covers a wide spectrum of topics: the cultural revolution and the role of the Red Guards; the Party and the evolving decision making and power structure in the village; the emerging pattern of finance and investment; the new remuneration system and the social security system; the reform in education and the rustication of urban educated youths; the health care program, including the much publicized "barefoot doctor" and the new medical insurance; the evolving farming practice and diversification; the revival of indigenous small-scale industry; women, marriage (including the regime's attempt to promote delayed marriage), birth control, and many other subjects.

The Myrdals' new work is more than a piece of first-rate journalism; it is also a passionate apology and plea for open-minded understanding of the cultural revolution. They do not pretend to be neutral; they readily admit their sympathy for and enthusiasm about what they heard and saw in Liu Ling. While their indictment of the Soviet and the Western press's distortion and misinformation (deliberate or otherwise) about the cultural revolution is generally well taken, their eagerness to defend the new programs leads at times to excessive enthusiasm and idealization. While it is true that Mao characteristically outlines broad policy goals and general direction, and leaves the details for the low level cadres to work out, encouraging the "masses" to exercise discretion and initiative in order to maximize *yin-ti chih-yi* (getting the best out of each area), it is definitely not true that the "masses" actually make decisions on the basis of "the living study and application of Mao Tse-tung thought" (pp. 192–93). The "masses" are by no means free to reject the "general recommendations" issued by the center in the name of Chairman Mao. One wonders what would happen if the "masses" indeed turned down the recommendations. It is one thing to talk about the importance of Maoist thought in contemporary China; it is quite another to say that it is Maoist thought that is holding China together.

Like their earlier work, one of the greatest merits of the present one is the faithful recording of conversations with the villagers. Herein also lies its drawback. As in the earlier work, the peasants in the present volume talk very much the way the official press reads. It is virtually impossible to determine whether they are expressing their own views and feelings or simply parroting the official line which, thanks to the Party's relentless "ideological work," they have thoroughly internalized.

Hsiung in his book attempts an "interdisciplinary approach." He draws freely on the insights, perspectives, and analytical tools of various social sciences from cultural anthropology to linguistic analysis. The result is an eclectic but refreshing analysis which synthesizes the findings of others yet offers the author's own perceptive and original interpretations.

Hsiung's work is in three parts. Parts One and Two interpret the Chinese Communist ideology, mainly but not exclusively Mao's thought, in historic and comparative perspective. Part Three is a competent summary of the major trends since 1949: the ideological and policy directions; the growing tension between ideology and practice; the evolving leadership conflicts over a wide range of fundamental developmental issues and concrete policy measures which found their ultimate expression in the cultural revolution; the Maoists' aims and tactics in launching the cultural revolution; the twists and turns of that revolution; the subsequent reshaping of the Party, the polity, and society in the Maoist image; and the emerging approaches to the development inspired by Mao Tse-tung's thought.

Hsiung is at his best when dealing with the nature, role, and functions of ideology. He observes that in China "ideology is never conceived of as having emanated from any segment below; the ideology is always imparted by the center from above," and presumably transcends social classes. "In Communist China ideology . . . seeks to establish a new culture, a new way of life, through purposeful, collective means. It is all-inclusive. It offers society an apocalyptic vision, a spiritual body of concepts and vocabulary for communications, and a methodology" (p. 7). He finds striking similarities between traditional and contemporary China with respect to the role of the ruling ideology. "The content of the prescriptive system has changed, but its function has not" (p. 8). Marxism-Leninism-Maoism has replaced the Confucian ethos, taking over the latter's function as the "culmination of what we in the West term social conscience, morality, religious faith, and constitutional law" (p. 7). One might add that while the *literati* of the past upheld Confucianism in order to rationalize and preserve the status quo, the Communist Party of today uses its own ideology in an attempt to transform China into a socialist modernized nation state.

Hsiung offers refreshing interpretations of the goals of Mao's revolution, the content of Chinese Communist ideology, and particularly of Maoist thought. Since he covers a broad range of topics, one cannot do full justice to his work in a short review. My comments will be confined to two points on which the author believes he has something new to say. The first is his perceptive discussion of Mao's evolving conception of "class struggle"; the second his unelaborated analytical distinction between two types of leadership, ideological and organizational. Hsiung has largely succeeded in the first task but, in my opinion, not done so well in the second.

According to him, class struggle has been a standard device employed by Mao to combat and correct the various cultural tendencies and traits he condemns (pp. 119–121). While persistently employing it as an instrument for social and cultural transformation, Mao has altered his ideas on the content and targets of class struggle over time. Until about 1960, his views were essentially in accord with orthodox Marxism; class struggle was "interclass, interpersonal, and outward-directed" against the class enemy, the "bourgeoisie," "the exploiting and oppressing classes" and so on. Since the early 1960's, it has been enlarged to "intraclass, intrapersonal, and inward-directed" (p. 209). "In the current usage, the term means the struggle within each person, irrespective of his class background, between proletarian thinking and remnant bourgeois thinking" (p. 122).

As for the second point, Hsiung's analysis leaves something to be desired. In spite of his initial claim (see p. 9), Hsiung never really elaborates on the analytical distinction between the two types of leadership. Although his subsequent analysis seems to identify Mao as a practitioner of leadership by ideology and Liu Shao-ch'i as a practitioner of leadership by organization, he never defines the two key terms, much less pursues their theoretical significance. I concur with his taking Schurmann to task for subsuming ideology under organization, and I am also persuaded by his argument that Mao has had a penchant for ideological leadership (whatever that means) which makes him insist that the organization be an instrument in the service of the ideology. I am not, however, convinced by the suggestion that Liu is a man who relies primarily upon organization and cadres rather than ideological work to carry out sociopolitical transformation. For if Mao's kind of leadership implies that the organization must be at the disposal of the ideology, then leadership by organization presumably implies that the organization may subvert or even transcend the ideology. To the extent that this is indeed what Hsiung means, one logical corollary of such an argument would be that Liu places organization above ideology. But in reality Liu was only employing the Party structure to achieve his own vision of a good socialist society, his ideology, which unfortunately deviated from Mao's. Furthermore, Liu's "On the Cultivation of A Communist," to which Hsiung also refers, offers ample evidence that he was as concerned with "ideological work" and "mass line" as Mao; he also stressed the cadres' commitment to the Communist ideology and moral integrity, upon which all Chinese Communist leaders base their claim to leadership. There is undoubtedly genuine difference between Mao and Liu with respect to leadership style, but it is a matter of proportion and emphasis; Hsiung's distinction between the two types of leadership is analytically ambiguous (at least to me). Insofar as such an analytical distinction can indeed be made, it must be more in the nature of Weberian "ideal type" with no empirical referents.

Pi-chao Chen

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The Green Shirts and the Others: A History of Fascism in Hungary and Rumania. By Nicholas M. Nagy-Talavera. (Stanford: Stanford University. Hoover Institution Press, 1970. Pp. 427. $9.95.)

This is a highly readable and, in spite of its many shortcomings, a basically useful book on the evolution and characteristics of fascism in Hungary and Rumania. The author is a Hungarian of Transylvanian background, who experienced the horrors of fascism in his homeland and spent seven years (1949–1956) in Siberian labor camps. His experiences with both forms of totalitarianism, his mastery of a number of foreign languages, and his academic background (following his arrival in America, he recieved a Ph.D. from the University of California at Berkeley) make him uniquely competent to tackle this morbidly fascinating subject.

Unfortunately, the book is somewhat of a disappointment. While the author may be technically correct in his claim that it is the "first more or less comprehensive and comparative work (on the subject) in the English language," much of the material has already been available to the readers in the West through the same major secondary sources on which the author tended to rely so heavily (e.g., Hilberg, Jaszi, Kállay, Lackó, Lévai, Macartney, and Roberts, to cite only a few of the major sources available in English). While it is useful in the sense that it brings under one cover the material pertaining to these two countries, it is quite unbalanced, for 6 of the 12 chapters (III through VIII) are devoted to Hungary and only 3 (IX through XI) to Rumania proper. The first two chapters provide a succinct historical background of the two nations and a review of the position of the Jews amidst them. The last chapter contains a comparative-analytical summary.

Although the book contains a wealth of useful information and a basically sound historical interpretation concerning the genesis and manifestations of fascism in Hungary and Rumania, its value as a scholarly reference work is questionable. This is due primarily to the author's carelessness in linguistic matters, superficiality in the handling of data, and tendency to engage in undocumented, sweeping generalizations. Aside from the many orthographic mistakes, the Rumanian names and titles are misspelled throughout the text to the annoyance not only of the purist, but also the researcher interested in verifying his data. Many of the historical figures and authors are misidentified. To cite just a few of many such examples, in describing the attack launched by the Black Front, a terrorist group of the Arrow Cross Party, against a number of Jewish worshippers, the text gives (p. 141) the date of February 1939 while in the footnote it is identified as February 4, 1937. The author Róbert Major is mistakenly cited as József Major (p. 104). The massacres at Kamenets Podolsk are identified as having taken place in July 1941, whereas they actually took place on August 27–28, 1941.

Perhaps the greatest weakness of the book lies in the author's unsubstantiated sweeping generalizations. For example, the author states that the ultra-rightist Szeged idea and Gyula Gömbös' rise to power were aided by "Jewish financiers" (p. 88) and that fascist agitation in Hungary was Jewish-financed (p. 89), as was the Hungarian Party in Czechoslovakia during the 1930s (p. 148). Such statements leave the impression (though this is absolutely not the intention of the author) that the Jews were largely responsible for the development of fascism in Hungary. Evidence should have been provided showing the absolute and relative numbers of these so-called "Magyars of the Jewish faith" (*Izraelita-vallású magyarok*), and their proportion and relationship to the Jewish community. Many of the members of the dozen Jewish families, who, according to Nagy-Talavera owned or controlled the "mammoth enterprises that composed about 90 per cent of Hungarian industry and banking" (p. 148) were in fact converts to or born into the Christian faith. Many of his other claims, as for example that "Horthy never desired or believed in German victory" (p. 180), or that Szálasi, the Arrow Cross leader, opposed the destruction of the Jews "to the very end" (p. 202), or that the Hungarian Government had explicit warnings and knowledge about Auschwitz" (p. 203) are all interesting, but in the absence of evidence not very convincing. This lack of investigative effort also leads the author to make a series of declaratory judgments concerning Nicolae Titulescu (p. 257), Sámuel Stern and the other members of the Jewish Council (p. 203 and 241), and Kasztner (p. 214–15). While the accusations relating to the former's corruption, and the latter's servility, spinelessness, and collaboration with the Germans may have some basis, the evidence is not supplied.

Nagy-Talavera properly devotes considerable space to the treatment of the "Jewish question," for without understanding it, one cannot possibly grasp the implications of the fascist phenomenon in these countries. He is rather superficial, however, in describing the historical background of the two Jewish communities (Chapter II) and basically inaccurate in his analysis of the era of the holocaust in Hungary. (The Rumanian Jewish community fortunately escaped the mass deportations to Auschwitz and other extermination camps.) His historical sketchiness may be due to his failure to exploit properly the vast Jewish historiography relating to the genealogy of these Jewish communities. His inaccuracy about the holocaust may derive from his reliance on a book by Lévai, the noted Hungarian journalist, which appeared as *Fekete Könyv* (Black Book) in 1946. While still a valuable source, it obviously could not anticipate the wealth of new archival and documentary materials that have been unearthed since its publication. These materials Nagy-Talavera has largely overlooked.

While the shortcomings of this book are many, so are its virtues. It includes a good chronological account of the fascist movements in Hungary and Rumania, skillfully tracing the similarities and differences between them to their divergent national-historical roots. It is particularly successful in demonstrating the basic identity of their inner core (ultranationalism and anti-Semitism) and the similarity of the social-political milieu in which they evolved and flourished. It is comforting to know that the faults of this book are easily correctable. This reviewer hopes that the author will manage to bring out a second, revised edition, for the important subject he tackles has long been neglected by serious scholars.

RANDOLPH L. BRAHAM

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Mongolia: A Profile. By Victor P. Petrov (New York: Praeger Publishers, 1970. Pp. vi, 179. $7.50.)

This timely item in the Praeger Profile Series is coincidentally available for the Fiftieth Anniversary of the Mongolian People's Republic. It holds no surprises for those already familiar with Mongolia, but it is a concise overview of the first communist nation in Asia and prototype of East European Russian satellites.

The book gives a good selection of the more important facts and figures about Mongolia. The book covers both past and present, but it offers little interpretation or analysis of the nation which is becoming increasingly more involved in world affairs. The first five background sections present an interesting historical summary but include a rather simplistic explanation of the rise of the great Mongol Empire —largely the old, unsympathetic, blood-and-thunder interpretation of Mongol expansion.

In discussing the epic of Mongolia's socialist revolution, Petrov emphasizes the invasion and counterinvasion of White and Red forces, but no mention is made of the popular discontent, the grinding debt of the Chinese merchants, the corruption of the Mongol elite, or the stagnation of the economy and the church—all of which were predisposing tendencies to revolution.

More informative are ten capsule treatments in separate chapters on such topics as government, economy, industry, agriculture, transportation and communications, the people, arts and recreation, vanishing customs, and foreign relations. Throughout the book Petrov correctly details how Russia has had the greatest influence of any foreign nation on modern Mongolia, but one wishes he had taken time to explore his complementary assertion that were it not for the Mongol invasion, the whole course of Russian history would have been entirely different (p. 5).

Throughout the book Petrov succeeds in impressing the reader with the rapid changes in Mongolia during the last fifty years. He notes the persistence of nationalism as well as a certain measure of independence from the Soviet patron. The study gives 1911 as the date of the first major penetration by foreign capital, but actually Chinese merchants dominated Mongolia economically long before this date. Terminology and proper names are generally accurate, but occasionally there is a lapse such as Huh Hoto (for Hohehot), Altay (for Altai) and Genghis (outmoded French form for Chingis). Mr. Petrov prematurely notes the end of Lamaism in Mongolia when, as matter fact, remnants persist; indeed, recent visitors have found some evidence of new growth with the establishment of a Lamaist academy and the recruitment of new candidates for the priesthood.

There is no indication of the major sources used in writing the book, but it is authoritative and without any distinctive bias. The treatment is generally sympathetic to the Mongols and will serve very well both as a guide for tourists and as a source book for the public schools.

Paul Hyer

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Social Fragmentation and Political Hostility: An Austrian Case Study. By G. Bingham Powell, Jr. (Stanford, Calif.: Stanford University Press, 1970. Pp. 201. $7.50.)

The notion that a fragmented society is incompatible with stable, effective, democratic government is a venerable one, but only recently has it been subjected to concerted, empirically based scrutiny. Professor Powell's admirable case study of social relations and political performance in an Austrian community carries this scholarly dialogue several important steps forward.

The hypothetical causal chain here is familiar. Where social cleavages—economic, religious, ethnic, and so on—are cumulative rather than cross-cutting, social and political organizations will tend to follow these primary cleavage lines, creating mutually hostile subcultures. The resultant intergroup enmity will exacerbate political conflicts, imperiling effective democratic government. Austria has long been considered a classic example of such a "centrifugal" polity: irreconcilable antagonism between the "Red" and "Black" *Lager* is said to have led inexorably to the collapse of the First Republic, and only the extraordinary "Grand Coalition" between the leaders of the two *Lager* is supposed to have averted similar difficulties after World War II.

Powell finds that at first glance politics in his community of Hallein conform to this pattern. Leaders of the two principal parties vilify one another, refusing to cooperate even in situations of high communality of interest. Innovative solutions to public problems are lost in a short-sighted struggle for narrow tactical advantage. Elaborate arrangements for reciprocal self-defense (the so-called *Proporz* system) constantly threaten to collapse, jeopardizing all stable government. Access by ordinary citizens to the political process is restricted by the rigidity of party ties. Even though the "load" on this political system is minimal, as Powell convincingly shows, the high levels of conflict severely impair the performance of the system. This is just the pattern of politics which the theory of social fragmentation is intended to explain, and using data on mass attitudes and elite behavior, Powell carefully examines each link in this lengthy theoretical chain.

Though partisan affiliation is closely associated with both social class and religious orientation, religion and class are themselves virtually uncorrelated, implying that in Hallein at least, these lines of cleavage are cross-cutting rather than overlapping. Still, as predicted, partisans in "pure" cleavage positions—upper-class Catholic supporters of the OeVP, and working-class Protestant or areligous supporters of the SPOe—display less tolerance for their opponents than do partisans in "cross-cutting" cleavage positions.

As in the rest of Austria, many secondary associations in Hallein have close ties to one or the other of the two main parties, and Powell finds that voters whose organizational memberships are limited to groups allied to their own party show considerably greater hostility toward their opponents than do members of associations cutting across party lines. Moreover, hostility is greater still among the leaders of "cumulative" organizations than among their less active colleagues. The impact on political enmity of organizational fragmentation along party lines is even more direct than is the impact of social fragmentation.

Thus, as predicted, individuals in "pure" social and organizational positions show more hostility toward political opponents than do cross-pressured individuals. But in Hallein both the absolute level of "purity" and the absolute level of partisan hostility are quite *low*. That is to say, although the data do tend to confirm the theoretical propositions linking social fragmentation and political hostility, they cast doubt on the view that Hallein is deeply fragmented, at least at the mass level.

At the elite level, on the other hand, Powell finds much higher levels of partisan enmity. This situation seems due in part to patterns of recruitment, in part to patterns of elite-mass interaction, and in part to the strategic imperatives of political competition. Mutual fear and hostility, even if found only at the elite level, tend to be self-perpetuating. Thus, like Lijphart, Stiefbold, Lorwin, and other recent students of "segmented pluralism," Powell finds the key to the system in the values and habits of elites and subelites.

The strength of Powell's work is the sophistication and breadth of his theoretical concerns and the care with which he explores the distinctions and propositions suggested by his data. He is not always equally careful in his marshaling of evidence nor equally sophisticated in his measurement techniques. His eagerness to illuminate theoretical propositions sometimes leads him to stress relationships which are statistically dubious. But Powell is here not "testing" theory, but "exploring" it, and such methodological deficiencies are not critical.

Since his "exploration" leads him to highlight the importance of elite perspectives, it is unfortunate that his examination of this factor is much less systematic than his analysis of mass attitudes, which turn out to be less significant. His examination of the correlates of partisan hostility at the mass level, for example, leads to some unexpected insights into the origins of this hostility, and similarly serendipitous discoveries might have emerged from systematic analysis of elite attitudes. Or another example: There are fleeting references to "norms of civility and tolerance" at the elite level, but we are nowhere given an assessment of the breadth and depth of elite adherence to such norms.

But perhaps the most serious weakness of this study is its surprisingly ahistorical character. Powell refers repeatedly to the impact of the past on contemporary attitudes and behavior, but aside from a brief, conventional summary of local history, he makes no attempt to look longitudinally at changing patterns of social cleavage, social organization, political attitudes, political conflict, or systemic performance. As Lorwin and others have argued, Austria (like other fragmented societies in postwar Europe) seems to be undergoing "defragmentation" (the Dutch have a word for it —*ontzuiling*), and many of Powell's conclusions—about the relative importance of elite and mass attitudes, for example—may depend on the particular stage which Hallein has reached in this process.

In fairness, it must be added that Powell seems to recognize these weaknesses in his analysis, and we may perhaps look forward to more intensive exploration of these facets of the problem in his subsequent work. This initial installment is an outstanding contribution to our understanding of the bases of effective, democratic government, of interest not merely to students of Austrian and European politics, but to anyone concerned with empirical theories of governmental performance.

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The Government of London: The Struggle for Reform. By Gerald Rhodes. (Toronto: University of Toronto Press, 1970. Pp. 320. $9.50.)

Gerald Rhodes, of the Greater London Group at the London School of Economics and Political Science, has produced a lucid and probing narrative study of "the struggle for reform" in London government, concentrating on the years from 1957 when the Government decided to appoint a Royal Commission on Local Government in Greater London to 1965 when the London Government Act of 1963 became fully effective. As the author reveals, the story of local government in modern London had earlier beginnings. The Local Government Act of 1888 created the County of London with jurisdiction over most of the built-up area of that day, and in 1899 the numerous lower-tier units in the county were consolidated into 28 Metropolitan Boroughs. Following these actions, the pattern of London local government remained almost undisturbed for six decades. In 1956 and 1957 the Conservative Government did hesitatingly address the question in two White Papers. Then, somewhat surprisingly, in July 1957 the Government announced its decision to appoint a Royal Commission.

The membership of this Commission (called the Herbert Commission for its chairman, Sir Edwin Herbert, now Lord Tangley) and its terms of reference provided clear signs of the Government's commitment to examine fully the issues of metropolitan boundaries and of local government centralization. None of the members had any inhibiting connection with local governments in the London area. The terms of reference gave the Commission power of review over the continuous built-up area of Greater London and the power to consider all the functions then being performed there by local governments. Thus armed, the Commission held extensive hearings, taking evidence from local government associations, national government ministries, political parties, professional bodies, and university groups. The greater part of this evidence supported the status quo or proposed quite limited changes; only the Greater London Group (Professor Robson, chairman) presented a comprehensive design for reform.

The unanimous report of the Herbert Commission (October 1960) proposed, in an effective presentation, that a Council for Greater London be established with jurisdiction over an area about 10 per cent smaller than the Commission's review area and that 52 Greater London Boroughs replace the approximately 100 existing local units. The metropolitan goal was thus substantially sustained; the centralization issue was resolved in two ways: the existing local units were reduced by half while the resulting larger boroughs were made the primary units of local government, the GLC being given few exclusive powers and made a weaker center than its predecessor, the LCC.

Reactions to the Herbert report were mixed, the greater portion again being hostile to change or proposing solutions much milder than the Herbert recommendations. Nevertheless the Government moved forward with a White Paper (November, 1961) which endorsed the main proposals of the Herbert Commission, invited responses from the affected local governments, engaged in a two-day parliamentary debate on the White Paper, made concesssions in boundary matters and in the arrangements for education, and finally (November 1962) introduced its London Government Bill. In this year-long passage from White Paper to London Government Bill, the metropolitan area was further reduced by about 15 per cent; the boroughs were made larger and reduced in number to 32, plus the City of London; the LCC education system was renamed but kept intact; and the whole London reorganization question became a major political party issue, with the Labour party in determined opposition. From introduction to enactment (July 1963) the Bill was in progress more than seven months, but its final provisions reflected no major change.

Considering the Labour spokesmen's charges that the London reorganization was a Tory plot to capture London, the first GLC and London Borough election results in 1964 were ironic: Labour won GLC by a ratio of two seats to one and got majorities in 20 of the 32 boroughs. (Subsequent London elections produced a different pattern: the Conservatives won GLC majorities in 1967 and 1970 and a majority of the boroughs in 1968, but in 1971 Labour came back to win a majority of the boroughs.)

The reform goal of metropolitanization was thus largely met; three-fourths of the London metropolitan region as defined by the Herbert Commission's review area was placed under the GLC umbrella. The centralization goal, never as clearly articulated, was somewhat ambiguously met: the local units were reduced to one-third their previous number, but this centralization was balanced by restricting GLC powers and by continuing the direct relations of the London Boroughs with national government ministries.

The consequences of these new and old arrangements for the effective governance of Greater London is to be the subject of a projected second volume by Mr. Rhodes; all students of metropolitan governments can therefore look forward in confidence to an equally thoroughgoing and illuminating appraisal of London government in action.

WALLACE S. SAYRE

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Nation-Building in Africa: Problems and Prospects. By Arnold Rivkin. Edited by John H. Morrow. (New Brunswick, N.J.: Rutgers University Press, 1969. Pp. 312. $10.00.)

For those who knew Arnold Rivkin and found his work thoughtful and useful, this posthumous work is a sad disappointment. It would be better to remember Mr. Rivkin by his earlier books and his work with the World Bank than by this plethoric collection of banalities. Anything in this book has been said better and earlier elsewhere, a point the author underscores by well-placed quotations that summarize what he has been trying to say in the preceding pages.

The mere mechanical problem of outdating is discouraging enough to begin with. Most of the detailed narrative, case studies, and initial papers or speeches on which this book is based date from 1964–1966; the Ghana coup had just occurred, the Nigerian military takeovers had just taken place. The author died in September, 1968; the book was edited by former Ambassador John Morrow and bears the publication date of 1969. This review was requested in March, 1971, and written in July, 1971, presumably over a year after the book appeared. The basic material is now half a decade old, presenting a strange game of mirrors for a study that aspires to look ahead into the near future. The Africa of 1966 or even of 1969 is not the Africa of 1971.

If the insights were new or brilliant or challenging, all such mechanical observations would be trivia. But the argument, a rebuttal to those Africans and Africanists who believed that the single party was the uniquely effective means of mobilization and development, reads like an evening talk to the Rotarians or an Army reserve seminar. For the most part, the developmental possibilities of African states are "uncertain," and the promised "continuum [of] . . . prospects" (p. 205) is no more explicitly presented than are the criteria or measures for establishing such a continuum. There is no doubt, however, that Nkrumah's Ghana is "bad" and federal Nigeria is "good," with the other states scattered in between, closer for the most part to Ghana than to Nigeria. Africa's chances at development are poor because Africa tends toward the "centralized, unitary, one-party, authoritarian-oriented, large public sector" end of the spectrum (pp. 205, 227, 257). But the relationship between centralization and development is only summarily asserted, never critically examined. Even the fact that "good," pluralist Nigeria broke down into a long and bloody civil war, while "bad," centralized Ghana merely underwent a comparatively easier military coup is never subjected to much analysis or reflection.

The central concept of the work is "nation-building," "the central process in the life of all the new states of Africa (as we have now defined it), which can be *consciously* directed or at least influenced and which will determine what they are to become" (p. 10). Whatever such a notion means, it seems to include the concept with which it is usually contrasted: state-building. Socialization and institutionalization are treated as inseparable, as is economic development. There is not enough precision in the central concept to argue with, analyze, or even simply restate.

A few ideas flit through, tantalizingly. There is the notion of devolution of power, which appears least fleetingly in the chapter on federalism in the discussion on local government. But how the strengthening of local power structures can contribute to the building of a nation is never probed. There is an unusual and intriguing notion about the relation between class and status divisions, with the suggestion that classless vertical divisions may be conducive to development (or nation-building) (pp. 244–245). But the potentially fascinating idea for speculation and analysis is not pursued. The author strongly asserts the virtues of free enterprise, pluralism, and agricultural investment for development (and nation-building), but the discussion is superficial and never makes any attempt to overcome counterarguments in a debate that has been around for a long time.

Although this book covers the continent, both functionally and by country, I am sorry to say that I can see no purpose it serves. It is not a textbook, a contribution to a great debate, an exposition of new material, or an analysis of timely events. It is a tired rehash of old saws, whose superficiality does not even do justice to the importance of many of the principles it propounds. Pluralism deserves a better defense; even the single party concept deserves a sounder attack.

I. WILLIAM ZARTMAN

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Haiti: The Politics of Squalor. By Robert I. Rotberg. With Christopher K. Clague. A Twentieth Century Fund Study. (Boston: Houghton Mifflin Company, 1971. Pp. 456. $10.00.)

This book can be divided into three distinct parts. The first is comprised of Chapters 1, 5, 6, and 10, and is supposedly the essence of the work: an analysis of the Haitian political system under Duvalierism. An attempt is made to formulate a theoretical framework characterizing Haiti as a "predatory" state—although the meaning of the term remains confused throughout the book. A predatory state is simultaneously described as "one in which brigandage is the predominant form of power, where effective power is exercised by praetorian specialists in violence who insure the safety of and respect for the formal center of power" (p. 342) and "as a species of what Harold D. Laswell and Abraham Kaplan (*Power and Society: A Framework for Political Inquiry* [New Haven, 1950]) called virocracy—the form of rule in which the elite is 'recruited in terms of well-being (vigor, vitality, prowess)' (p. 211)" (p. 342n). The author, who has been in Haiti only a few days during past years, has arrived at various sweeping conclusions about the psychological character of the Haitians. He claims that most Haitians show signs of paranoia and that most Haitian political problems derive from certain putative child-rearing patterns.

America was not familiar to Alexis de Tocqueville before he visited it and wrote his lasting work, *Democracy in America.* But de Tocqueville travelled intensively throughout the United States to observe the country and its people. He kept himself from reading what others had published about it for fear of being unconsciously influenced by them. In writing about Haiti, however, Rotberg proceeded in just the opposite way. His attempt to conduct field research having been frustrated, he collected *everything* written about Haiti he could put his hands on, hired five research assistants and two translators, and held seminars on Haiti at which selective issues were debated. As a result, the book is not the work on national development it pretends to be but is, instead, an instant historiography similar to the many published yearly in the United States analyzing the policies of current American presidents. The wealth of trivial details, intrigues, and rumors reported throughout Rotberg's text help keep it on the level of the lengthy journalistic accounts already published about Haiti (i.e., Diederich and Burt, *Papa Doc,* etc.). For readers familiar with Graham Greene's novel, *The Comedians,* Rotberg's accounts will seem *déjà vu* and quite dull.

The second part of the book—Chapters 2, 3, and 4—is a survey of Haitian history. It is well developed chronologically, but the inaccuracies are too plentiful, and the space allotted to this review too short, to allow this reviewer to rectify them. Rotberg's skimming survey only sketches the events that had made the advent of a Duvalier almost inevitable. The Duvalier phenomenon has been entirely misunderstood by the American press, the United States State Department, and Rotberg's other sources of information. This stage was one that Haiti had to go through in order to make socioeconomic development possible. Although the old elite was soon replaced by a new one, the new elite is clearly more open, more representative, more responsive to the rest of the population. Now that François Duvalier is dead, that his son Jean-Claude is the thirteenth President-for-Life, and that a very competent and reliable group of technocrats are working to achieve the goals of Duvalierism, "Papadocracy" seems, in retrospect, to most Haitians to have been a necessary transitional stage. (This view does not prevail, however, among the small circle of exiles who were displaced either by the Duvalierist social revolution or by the violence that accompanied it. These exiles are as far out of the mainstream of the system as are the Cubans who left Cuba under Castro.)

The third part of the book—Chapters 7, 8, and 9—deals with human resources, the structure of the economy, and policies for economic development. This part, presumably written by Professor Clague, is the only one that seems a worthy contribution to the goals of the work. These three chapters alone would have constituted a good guide for scholars or foreign government officials interested in assessing the need for external support for the development of Haiti—if they were not lost in the midst of trivia.

A good analysis of the Duvalier years is needed to fill a void because, like Argentina's Perón and Brazil's Vargas, Duvalier has been one of the most important landmarks in the development of the Haitian political system. From now on, the history of Haiti will be analyzed in terms of the periods before, during, and after François Duvalier. The violence of his regime will not be ignored (nor does one ignore that violence under Perón or Vargas), but the unique social changes brought about by the regime will determine forever the course of Haitian economic development. Rotberg's book leaves the void to be filled by a more serious study.

JEAN-CLAUDE GARCIA-ZAMOR
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Scandinavian Political Studies, Volume 5/1970. A yearbook published by the Political Science Associations in Denmark, Finland, Norway, and Sweden, Chief Ed.: Olof Ruin. (Oslo: Universitetsforlaget; New York and London: Columbia University Press, 1970. Pp. 366. $10.00.)

In 1622 at the University of Uppsala Chancellor Johan Skytte endowed from his personal funds a chair in Wellspokenness and Knowledge of State (*vältalighet och statskunskap*). He had earlier been tutor to Gustaf II Adolf, the first king to make Sweden into a great power. In providing for the training of young diplomats in rhetoric and politics, Skytte created what was to become the oldest academic tradition in political science.

Rudolf Kjellén, the founder of geopolitics, in 1923 was succeeded as Professor Skytteanus by Axel Brusewitz whose students in a score of dissertations composed a unique compendium on parliament-cabinet relations throughout Europe, and in a dozen seminar papers compiled the first comparative study of the social composition of legislatures. Brusewitz's *docent*, Herbert Tingsten, in 1937 published a quantitative study of comparative electoral analysis whose title, *Political Behavior*, became the password for a generation. More recently Stein Rokkan, at an institute endowed by a Norwegian shipping magnate and statesman, Christian Michelsen, has become a leader in combining sociological, statistical, and historical perspectives on politics.

In the last twenty years new universities and larger cadres have provided long-needed opportunities for younger scholars. Still, political science in Scandinavia remains intimate enough to allow a scholar to move from political philosophy to quantitative political sociology, or from public administration to international relations.

Scandinavian Political Studies, appearing annually since 1966, provides a selection of original essays in English featuring the best of political analysis by or about Scandinavians. Among the current contributors are five Swedes, three Danes, three Norwegians, two Finns, and one American. Most articles include extensive bibliographies and run to about 15,000 words, which gives scope for sustained argument and well-marshalled data.

Two opening essays examine concepts current in the discipline. Erik Rasmussen (of the University of Aarhus) finds Easton's notion of "political system" more adequate than the critics have allowed. Lars Rudebeck (Uppsala) finds fault with the concept of political development as propounded by Almond and Powell (who get a C), by David Apter, and by his fellow Scandinavian Erik Allardt (both of whom get B's); and pleads for a "logically coherent . . . approach" that is explicit about its "historical and normative . . . premises." Rudebeck's own historical materialism tends to reduce political to economic problems. Dismissing "political modernization" as a concept "narrower" than, but virtually "interchangeable" with political development, he overlooks the work of Black, Deutsch, Huntington, Moore, Rustow, and others who, under the heading of modernization, have treated just those historical, normative, and international aspects of political change that Rudebeck is rightly concerned with.

The third essay, by Stein Rokkan, is entitled "The Growth and Structuring of Mass Politics in Western Europe: Reflections of [i.e., on] Possible Models of Explanation." The models on which he reflects are those he has built separately in various articles over the years (most of them collected in his *Citizens, Elections, Parties*): (1) A model explaining "the sequence of steps in the institutionalization of formal mass democracy"; (2) one explaining "the timing of the growth . . . of organizations for mobilization of mass support"; and (3) a "unified model" that pulls together these two strands of government and politics, of democratic institutions and mass parties. To integrate these models, he insists, we need "time-specified variables"; and to arrive at a parsimonious list of such variables Rokkan himself proceeds by "paired comparisons." He offers one such comparison between Ireland and Finland, the two ex-colonies among European democracies—and it proves a brilliant sketch. For example, Rokkan suggests that the phased sequence of political crises in Ireland in the last century ("the cry for justice for the Catholic religion, the cry for land, and the cry for . . . independence") had much to do with preventing the kind of polarized class politics characteristic of Finland.

The upshot of Rokkan's article is a two-and-a-half page table that features 31 independent variables, concerning the territorial, cultural, economic, and organizational configurations of the political struggles in Europe since the sixteenth century, and 12 dependent variables comprising the patterns of democratization since the nineteenth century and of party alignment in the twentieth. As Rokkan recognizes, there is implicit here a major "challenge to comparative political analysis: the great question facing the student of comparative development is whether he can immerse himself so thoroughly in the details of national political history and still retain his primary interest in the parsimony of an overarching schema of concepts and propositions" (p. 66). If any one can rise to the challenge—and rise to it both in lucid prose and in computer printouts—Stein Rokkan can.

There follows a suggestive study of "Local Budgeting, Decision-Making or Ritual Act?" by Johan Polsen (Bergen), based on a Norwegian locality and making fullest use of what we have learned from political scientists on budgets, sociologists on organization, and anthropologists on ritual. "Sex Roles in Politics" by Elina Haavio-Mannile (Helsinki) similarly compares the Finnish evidence on women's voting with what is known from other countries and with available hypotheses by Duverger, Lipset, and others. (Finland, it is worth recalling, gave the franchise to women in 1906, preceded by New Zealand in 1893.) The next three essays take up electoral and parliamentary behavior in Scandinavia. Risto Sänkiaho (Helsinki) applies the Mann-Shapley index to party combinations in Finland. Nils Stjernquist and Bo Bjurulf (Lund) offer a computerized roll-call analysis of party discipline within and voting coalitions among the five parties in the Riksdag. Bo Särlvik (Göteborg) analyzes the Swedish elections of 1964, 1966, and 1968—a period of steady gain for the Social Democrats. Finally, Egil Fivelsdal (Copenhagen) and John Higley (Texas), in a long essay, find that the leaders of the Norwegian Federation of Labor comprise "a relatively closed corporate structure" whose "social backgrounds effectively distinguish them from other key elites" and who "reach power . . . through long and complicated career patterns and mechanisms which ensure experienced, politically seasoned 'insiders'" (p. 204).

A separate section entitled "Reviews" includes concise summaries of recent Scandinavian events of interest to political scientists: Norway's "first regular 'ins-outs' election in September 1969"; the Danish referendum of 1969, which rejected a Folketing measure lowering the voting age to 18; and "The Zig-Zag Course of the Nordek Negotiations." ("Nordek" is the organization for Scandinavian economic cooperation proposed by Denmark after de Gaulle's second veto of British entry into the EEC, and Claes Wiklund details the course of negotiations as well as the relevant data on public opinion and attitudes of organized interests.)

Like its predecessors, the volume concludes with a bibliography (406 items this time) of books, pamphlets, and articles by Scandinavian political scientists or about Scandinavian politics, with titles given both in the original and in English. Among the items I added to my own reading list are a study by E. Wittenberg (in German) on Bismarck's image in Weimar Germany; by J. Galtung, *et al.* (in Norwegian) on Gandhi's political ethics; by S. A. Söderpalm (in Swedish with English summary) on big business and the Swedish transition to democracy; by Vilho Lukkarinen on *The Role of Air Power in a Nation's Total Capability and in International Society* (Helsinki, 1969); and by C. G. Widstrand *et al.* on *African Boundary Problems* (Stockholm, 1969). If I knew Finnish, I would turn at once to A. Talikka's dissertation on "Frustration and Democracy: A Sociological Study of Frustration Situations as Preconditions of the Strategies in Political Conflicts" (Tampere, 1969). It is a theme that could not be more relevant in Finland than it is right here in the United States.

A perusal of *Scandinavian Political Studies* provides a welcome reminder that the systematic study of politics can be a truly international endeavor. European scholars in the last two decades have fully mastered American standards of empirical accuracy, explicitness of concept, and quantitative precision. Americans for their part would do well to emulate the philosophic breadth and historical depth that enriches the work of their colleagues in Europe.

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The Ministry of Housing and Local Government. By Evelyn Sharp. (London: George Allen and Unwin Ltd.; New York: Oxford University Press, 1969. Pp. 253. $5.50.)

This is the fourteenth of the New Whitehall Series of descriptive monographs of the government departments of the United Kingdom. Each is ascribed to the Permanent Secretary, though whether some are the outcome of the collective drafting, in which the British Civil Service excels, is a matter for the reader to determine. This one is ascribed to Evelyn Sharp, one of the outstanding women civil servants in whose work the British service also seems to excel, and its personal style indicates that Lady Sharp, Permanent Secretary until 1966, is indeed the author. Unfortunately, the format of the series severely limits the scope of the study, so that the author is inhibited from making a full survey of the relationship between central and local government. This is indeed unfortunate because given her long experience and intimate knowledge of British administration, a wider study and evaluation by Lady Sharp of this significant aspect of government would have been valuable and fascinating.

Within these limitations the book discusses in detail the problems and the main strengths of English local government, which are especially interesting to American readers because of the contemporary strains and stresses in the relationship among federal, state, and local government. The English use of central government as a means of ensuring uniform and reasonable treatment for the individual citizen, implicit throughout the whole range of the Ministry's activities and reinforced by the regular process of parliamentary criticism, contrasts with the contemporary reaction in the United States against Washington and all it stands for. The relative ease in achieving compromise and consensus in English government is frequently implied, but the author deals frankly with areas of difficulty and disagreement, which as she indicates can sometimes be both violent and protracted.

The value of the study to the scholar is the balanced presentation of each area of the Ministry's functions, and the specialist will find each section largely self-contained, with cross-references where necessary. There is an introductory historical chapter and a concluding description of the staffing and organization of the Ministry, which has recently been merged into a new Department of the Environment. The chapter dealing with the organization of local government is necessarily incomplete because local government in England is passing through the most important phase of reorganization since the County Councils Act of 1888, and neither the necessary legislation nor the process of reorganization will be completed for another three years.

There is a particularly interesting chapter on housing, which describes the way in which, over several decades, the municipalities have become more and more deeply involved and suggests that within a relatively short time all rented housing, city apartment buildings excepted, will be in municipal hands. In housing the limitations on the Ministry seem greater than in any other area of its activities, yet it has important responsibilities to the citizen for controlling standards, costs, and priorities.

The chapter on public health should encourage defenders of ecology and is an object lesson in the English flair for achievement by the inevitablity of gradualness rather than by confrontation. The author suggests that "Britain was the first country to make a determined effort to reduce industrial pollution and still leads the world. . . . The air over the cities of Britain is infinitely cleaner than it was—and will become cleaner still." These advances, begun as long ago as the Alkali Act of 1863, were extended and consolidated in 1906 by further legislation which gave the Minister powers to administer by inspection, now strongly reinforced by a Clean Air Act of 1956. The first Rivers Pollution Prevention Act dates back to 1876, although the real drive in this area began with a White Paper, issued in 1944, which proposed *A National Water Policy*. This led to further legislation: the Water Act of 1945, the Rivers (Prevention of Pollution) Act of 1951, and the Water Resources Act of 1963. River Boards were first established in 1948. The 1963 Act went much further by setting up a Water Resources Board and by replacing the local Boards by twenty-seven River Authorities.

The section on planning gives a useful and comprehensive explanation of what is involved in town and country planning. Beginning with the legislation of 1909, planning has been advanced by a series of Acts, the most recent passed in 1962, and has gradually evolved into a full-scale regional effort. The author does not in any way underestimate the difficulties and intricacies of the problem. She also discusses the relatively slow progress of national parks in Britain and gives a good account of the New Towns and all that has been involved in their development.

FRED TICKNER

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Religion and Change in Contemporary Asia. Edited by Robert F. Spencer. (Minneapolis: University of Minnesota Press, 1971. Pp. 172. $6.50.)

The seven essays which comprise this collection were first presented as a series of lectures at the University of Minnesota in 1968. Thus it is hardly surprising that the essays treat very different problems, and collectively provide little in the way of answers to the comparative questions which their juxtaposition naturally suggests. Apart from the very general theme stated in the title, there is only a nine-page introduction by the editor to tie them together. Most of the essays provide useful syntheses of what has previously been written on their respective subjects, and several contain stimulating insights.

In one of the best chapters, Arthur F. Wright analyzes the decline of Buddhism in China during the past fifty years. In addition to the hostility of successive Republican governments and the stigma of wartime collaboration with the Japanese, the Buddhists failed to articulate a meaningful social ethic and a world-view congruent with modern science. The rapid disintegration of Buddhism under Communist rule must be seen as a culmination of trends perceivable in the preceding decades. Marxism-Leninism, like Neo-Confucianism before it, constitutes a total coherent system, and therein lies its appeal to modern Chinese man. Buddhism, on the other hand, "offered no dream of an ideal society since, under its tenets, there could be no such society in a world of illusion and suffering" (p. 25).

Joseph M. Kitagawa's survey of the background of the new religions in Japan is heavily weighted on the side of the historical, tracing the development of Shinto, Confucianism, and Buddhism in varying relationships to each other. The new religions which have sprung up since 1945 are characterized as doctrinally simple and eclectic, mainly led by charismatic founders. Their holistic world views imply a basic unwillingness to divorce religion from politics, and hence the remarkable political phenomenon of Soka Gakkai.

In "Some Religious Components in Vietnamese Politics," I. Milton Sacks analyzes the four major religious groups in South Vietnam—the Cao Dai, the Hoa Hao, the Roman Catholics, and the Buddhists—in terms in their involvement in the political process. The first two—syncretist sects founded in 1919 and 1939 respectively—functioned as powerful political and military forces until their private armies were virtually destroyed by Diem in 1955–56. Sacks's treatment of the Catholics and the Buddhists is very brief, presumably because the political monks in the anti-Diem movement and subsequent events are much better known.

Agehananda Bharati's essay, "Hinduism and Modernization," develops the theme that in contemporary India the process of Sanskritization—the emulation of high-caste customs—is the phase of culture change prior to modernization. Those thoroughly Sanskritized, i.e., Brahmans and the other high castes, go on to the phase of secularization and modernization. Bharati then turns to a discussion of the *modernization of Hinduism,* by which he means the whole movement of modern Hindu apologetics and religious reform, epitomized in the work of Vivekananda. Bharati's numerous and not always illuminating anecdotes make his essay twice as long as the average in this volume. The ludicrous extremes to which some Hindu apologists have gone in attempting to establish the validity of their tradition in the modern world is a point worth making, but it is surely belabored in this essay, chiefly, one suspects, for its entertainment value.

Manning Nash discusses the Burmese experiment in the use of Buddhism for national revitalization. Nash emphasizes the divergent consequences of this involvement—political effectiveness and developmental stagnation:

> The political successes of U Nu were in large part due to his ability to mobilize Buddhist sentiment, to display traditional piety, to embody the religious life, and to exemplify the virtues of the middle way. However, his kind of revitalization of Buddhism did nothing to make it more rational, more modern, more conducive to the urgent problems of national consolidation and economic recovery and growth (p. 120).

The relationship between Islam and democracy in the Indo-Pakistan subcontinent is the subject of Aziz Ahmad's interesting contribution. The nineteenth-century Muslim modernist reaction to the idea of democracy was instinctively negative, for any form of majority rule meant the political dominance of Hindus over Muslims. In the twentieth century, however, Muslim spokesmen increasingly reconciled themselves to the growth of democratic institutions, while pressing for safeguards for the minority community, and ultimately, of course, for a separate Muslim state. Ahmad analyzes the claims made by modernists and apologists that early Islam contained the core of political democracy in the principles of *shūra* (mutual consultation) and *ijmā* (consensus). While he does not emphasize the point, it is clear that he regards these interpretations as the efforts of men forced to defend their tradition on foreign terrain. That is, the West provided the basic assumptions regarding the value of democracy, and Islam had to come to terms with it.

Ahmad then goes on to inquire into the relationship between Islam and democracy in Pakistan since 1947. He discusses the role of the *ulama* in the process of constitution making, and points out that some traditionalist and fundamentalist political ideas were "absorbed or adopted with mental reservations in the constitutional documents" (p. 139). In practice, however, all administrations have sought to mold Pakistan into a modern national state—but not into a modern *democratic* state. The failure of democracy is clearly not attributable to the influence of Islamic ideas. The military government which came to power in 1958, and the military regime which succeeded it, must be explained in secular terms, and these phenomena are common to many countries of the Third World, non-Muslim as well as Muslim.

One can only agree with Professor Ahmad as far as he goes. There is, however, a more subtle question to be asked: To what extent has the political culture of Pakistan been molded by a religious tradition with strongly authoritarian elements? While no conclusive answers are possible, it is worth considering the extent to which authoritarian value systems encased in religious tradition lend support to authoritarian political regimes. It is necessary to consider not only explicitly political ideas, but the general attitudes which underlie political behavior.

In the concluding essay Robert R. Jay refers to precisely this attitudinal element in his discussion of religious and political conflict in Java. Jay starts with the fact of the 1965 massacre of half a million Communists and their supporters, and then probes Indonesian history for clues to the ideological fanaticism which produced violence on such a scale. He finds a significant pattern of this phenomenon in the several centuries of conflict between orthodox Islam and Javanese syncretism. The pattern of ideological fanaticism did not "cause" the massacres of 1965, but it certainly helps to explain them.

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Documents of Korean Communism 1918–1948. By Dae-sook Suh. (Princeton: Princeton University Press, 1970. Pp. 503. $15.00.)

Although the Korean Communists organized a party as early as in August, 1919, the study of Korean Communism had been a scholarly virgin land for all practical purposes until Professor Suh's work, *The Korean Communist Movement 1918–1948*, was published in 1967. The current volume, *Documents of Korean Communism 1918–1948*, contains sixty-six documents which constituted the main basis of the former volume. For his study of Korean Communism, Professor Suh collected some 124 documents from various archival sources, and selected only about a half of them for this latter volume on the ground of what he calls "contextual importance to the (Communist) movement as a whole." These documents are arranged in six parts or stages, largely in chronological order: the early stage, 1919–25; the Communist parties in Korea, 1925–35; the Communist International, 1928–35; the activities abroad, 1930–44; and after the liberation, 1945–46.

In making the selection of documents to be presented, the author is keenly aware of the problems and possible distortions arising from the political division of Korea today. "The fiercely anti-Communist South condemns Communism in Korea, past and present alike, and does little to study Communism." On the other hand, the Communists in the North, without many ties with the old Korean Communist movement, dismiss their predecessors' efforts as "paltry, having little or no significance for the present." The author notes that "in past studies by the Koreans and the Japanese, only two or three documents, usually of Comintern origin, were appended either to condemn or to glorify Communism in Korea" (ix).

In a masterful effort to place the selected documents in proper perspective and explain their historical significance, the author begins each of the six parts with a succinct commentary which provides an indispensable historical background—though unavoidably fragmentary. Some of the documents would be nearly unintelligible without these informative commentaries that only someone with author's extensive knowledge of Korean Communism could write.

Also useful are the biographical sketches of many key figures in the Korean Communist movement, e.g., Pak Chin-sun (pp. 12–13) and Pak Hŏn-yŏng (pp. 480–82). Some of these materials provide fascinating insights into little known aspects of the lives of some Korean political leaders who are not known primarily as Communist activists, such as Kim Kyu-sik (pp. 17–18) and Kim Tu-bong (pp. 482–85) whose revolutionary activities began under the Korean nationalists—not Communists.

It is remarkable that this volume, essentially a compilation of documents, biographical and reference data interwoven with straightforward commentaries, succeeds in presenting a comprehensive picture of the tortuous evolution and convulsions of the Korean Communist movement during three decades. The book manages to convey the agonies of Korean Communism—agonies that partly reflected the hapless fate of the Korean peninsula under Japanese occupation and the rootless vagabond existence of many Korean Communists who were barely tolerated in China and Siberia. It is not surprising that a recurring theme of many documents from the early stage of the Korean Communist movement reads:

> In order to secure the normal spiritual and physical development of the Korean toiling masses it is necessary to free Korea from the yoke of Japanese imperialism and capitalistic exploitation (p. 48).

Thorny paths of the Korean Communist movement are also recounted in many painful incidents, such as the Alexeyvsk Incident of June 1921 in which the factional struggles of two Korean Communist cliques, the Shanghai and the Irkutsk groups, burst into an armed confrontation in the Russian Maritime Province. The problem of factionalism is shown to have been a persistent *malaise* that sapped the vitality of the movement. A document written by the late Otto Kuusinen in 1931 lamented in part:

In many parties factional skirmishes were taking place for years. There are parties that have become (universally) notorious in this respect such as the Polish and American parties, but the record belongs to the Korean factions (p. 271).

In fact, internecine struggles among the Korean Communists—undoubtedly intensified due to their lean and parasitic existence in inhospitable alien lands—caused much difficulty with the Comintern. Although the Korean Communists were organized as the Korean Communist Party in 1919, that party did not become a member or a section of the Communist International until 1928.

Documents presented by Professor Suh also reveal some new findings and new insights into the politics of Korean Communism. One notable example, introduced in the author's commentaries:

> Despite the commonly accepted notion that the Russians masterminded the coalition between the Communists in the north (North Korea) and the Yenan groups, the coalition was actually initiated by the Yenan groups, the North China Korean Independence League which changed its name to *Shinmin-tang*. This is made clear by the next document (p. 478).

One criticism of the work is that the author should have double-checked and edited the translations of some documents for the sake of readability.

Professor Suh states his hope that the importance of this study will lie not merely in the presentation in English of hitherto unavailable documents of Korean Communists, but rather in the formulation of a broader framework of inquiry. Using this framework, scholars may be able to interpret the existing documents more intelligently, uncover as yet unavailable key documents, analyze in detail various segments and phases of the Korean Communist movement, and thus enhance the understanding of Communism in Korea. The volume succeeds admirably in whetting serious interests in the study of Korean Communism by providing scores of key documents. Professor Suh's efforts have produced an indispensable reference work for the study of the Korean Communist movement.

JOHN KIE-CHIANG OH

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Social Stratification in Africa. Edited by Arthur Tuden and Leonard Plotnicov. (New York: The Free Press, 1970. Pp. 392. $10.00.)

The editors of this symposium cite the lengthy list of comparative studies produced by social anthropologists, to which list their own volume is intended as a positive contribution. But as they rightly state, "scholars have been relatively little interested in stratification as compared with kinship and political organization, as a review of past research in Africa indicates" (p. 2). The editors argue correctly that stratification is related to the "dynamics of social change" (p. 2). In this volume they have collected an impressive set of studies by very well known social anthropologists, all, with one exception, now teaching in American universities. Thus Aidan Southall writes on the Alur, Arthur Tuden on the Ila, James Vaughan on the Marghi and neighbouring West Sudanic peoples, Jacques Maquet on Rwanda, Allan Hoben on the Amhara and Ronald Cohen on Bornu. Each of these has presented an account of the ethnographic present—the traditional patterns of stratification observed during field research in the past two decades. Three authors—Leonard Plotnicov, Clyde Mitchell, and Pierre van den Berghe—write respectively of modern urban situations in Jos (Nigeria), Central Southern Africa, and South Africa.

Yet the book is a great disappointment. The ethnographic data are certainly valuable, but the theoretical approach adopted by most authors, and in particular by the editors themselves, seems to derive from the most sterile approaches of western sociologists to social stratification. If one had hoped that social anthropologists would bring a breath of fresh air into this field of study, he will not find it in this volume.

The editors seem to be on the right track when they state that "present theories, definitions and conceptual frameworks of social stratification have proved inadequate for dealing with African conditions" (p. 6); but then they continue with the amazing statement that this is because "within [African] societies great differences in wealth or control over the means of production are absent." Notwithstanding, the editors state that they are not concerned with social ranking or with the universal inequality of the distribution of rewards in society. Their concern is solely with stratified societies, that is to say "societies composed of ranked social groups that are organized on bases other than kinship statuses or biological factors" (p. 8). By this definition not all African societies are stratified. Several contributors in the volume discuss societies which we would all agree are stratified—the Rwanda or Amhara, for instance. But to include the Ila in this volume seems to be stretching a point, for the Ila are clearly an egalitarian people among whom slaves are easily absorbed; the division between free born and slave justifies inclusion, yet could not almost every traditional African society be included on such grounds?

The result of this approach is a static study of the character of these social groups and the comparison between some of the almost infinite variety of patterns which occur in Africa. Problems of definition dominate the editors' Introduction. That hoary question—Should "caste" be used solely for Indian societies?—is raised. How, the editors ask, does caste differ from feudalism, for Rwanda is described as a caste society and Amhara as feudal? Similarly, definitions of slavery and class are briefly raised and also the distinction between social stratification and social pluralism.

What one misses, both in the editors' Introduction and in most of the subsequent contributions is any discussion of the manner in which the dominant group maintains its position in the society, that is, of the power relationships in the society. Missing, too, is a clear distinction between the pattern of stratification as seen by the ethnographer and that perceived by the people themselves in the various societies—and in particular the differences between the perceptions of those in superior and in inferior groups. For a much more penetrating analysis of traditional African society the reader might turn to Ladislav Holy, ed., *Social Stratification in Tribal Africa* (Prague: Czechoslovak Academy of Sciences, 1968), which, significantly, is not mentioned in the volume reviewed here.

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Kultura Essays. Ed. by Leopold Tyrmand. New York: The Free Press, 1970. Pp. xi, 344. $8.95.)

As befits a nation distinguished neither by wealth nor power but which regards itself as more cultured than its domineering neighbor, there are two Polish periodicals called *Kultura:* one in Warsaw and another in Paris. The latter is unquestionably the best periodical in Polish: beyond the reach of censorship and at the same time free from the clericalist and nationalist narrow-mindedness and morbid introversion which mar so many exile publications.

The selected essays under review provide a fair sample of the kind of material which appears in *Kultura;* and while they show that it is one of the most highbrow general periodicals in the world, they hardly add up to anything that might be called a book in any but the material sense. The articles on art and literary criticism call for no comment here, since only the one on the purges of the Ukrainian writers is of direct political interest. Though moving, the eyewitness accounts of the Hungarian and the Czech tragedies, and of the death of an old Bolshevik contain no new factual information. The author of the highly intelligent and sensible (though not very original) general article on ideologies—Alexander Herz—can hardly be blamed for having accepted the prevalent view about the end of ideology, although it is to his credit that he was less sure about it than most people at that time. As all its heroes have already faded into unimportance, Z. A. Jordan's massively erudite article on revisionism is by now of interest only to specialized historians who can read the book in English of which it forms a part. As Stawar's book is only available in French and Polish, the extracts included here (though not very well chosen) will be useful to the English reader, especially on account of their history. When Stawar was given a state funeral in Warsaw, the bosses did not know that he had asked *Kultura* to publish this manuscript. Although it contains a number of intelligent remarks for the benefit of elderly Polish exiles prone to hallucinogenic nostalgia, the last and longest article in the collection suffers itself a bit from this very tendency, as its author does not seem to realize that "a political line" of a tiny group of exiles will not make the slightest difference to the march of events, and therefore is of little interest to anyone else. *Kultura's* more important task is to keep up the Polish cultural and intellectual standards in the fields which are stifled in Poland.

There is one article in the collection, however, which makes it a necessary item in every library where recent political history is studied: namely, Miedzinski's account of how he negotiated with Stalin through Radek on Pilsudski's behalf, and how Pilsudski tried to organize a preventive war against Hitler.

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Power and Conflict in a Mexican Community: A Study of Political Integration. By Antonio Ugalde. (Albuquerque: University of New Mexico Press, 1970. Pp. 193. $10.00.)

This study of political dynamics in the medium-sized Mexican city of Ensenada represents a refreshing addition to the growing body of literature on Mexican politics, an area of research in which interpretation has often outrun data. One of the few available empirical studies of politics on the community level, the book contains a wealth of detailed information on the organizational life of the community's residents, along with welcome attention to informal aspects of the political system such as corruption and the daily routines of government bureaucrats. The liberal use of illustrative quotes and cases greatly enhances the study's readability.

Unfortunately, this rich descriptive material is only loosely connected to a central thesis, making the study's argument hard to follow. As the author indicates, Ensenada cannot be regarded as a typical Mexican town because of its privileged tax status as a duty-free zone, its image as a frontier town filled with opportunity, its rapid growth by migration, and its proximity to the United States. Features of Ensenada's politics such as the existence of a strong opposition party (PAN) and a lack of polarization along interest lines are at times attributed to the community's unique location and social characteristics and at other times presented as the leading edge of a trend that will spread as other Mexican communities reach Ensenada's relatively high level of economic prosperity. Similarly, Ensenada's power structure is described initially as a microcosm of national politics containing all major groups (except student organizations) and revealing "the essential components of conflict and the essential processes for resolving these conflicts in the Mexican system." As the book develops, however, Ensenada emerges as a rather distinctive local situation in which labor and political organizations bear little ideological resemblance to their national counterparts.

Perhaps the most serious shortcoming of the study is its assumption that conflict is inherently dysfunctional and ought to be avoided at all costs. A conservative value judgment is smuggled in along with this assumption, since conflict resolution is one side of a coin whose other side is preservation of the status quo. Any given social system will serve the interests of some of its members better than others and thus will not be equally functional for all. If the system involves great inequalities in the distribution of wealth, political influence, and access to basic social services, then conflict may be highly functional from the point of view of the disadvantaged, even though it costs the social system some stability and integration.

The author's commitment to the concept of functional integration seems to prevent him from drawing some obvious conclusions from his data. The political system he describes is dominated by a relatively small group of prominent economic and government figures plus the "co-opted" leaders of various organizations. Some of these leaders represent segments of the population whose interests conflict, but the leaders themselves are in agreement on many issues and capable of a high degree of cooperation. The elite (our term, not his) is linked together in a well-described system of political brokerage that has "reduced social tensions and enhanced the integration of the society" (p. 180). This integration, however, is achieved at the expense of the poor, who remain both politically marginal and economically deprived despite their nominal membership in organizations that formally represent them and engage in sporadic "welfare" activities and "good will gestures" to maintain their support.

The author maintains that "the lower economic groups . . . are not very concerned about the quality of [public] services; they are pleased if their children can go to school, if they receive some medical attention, if they have water piped into their homes for four or five hours at night" (p. 129–30). They do not, he reports, regard violence as a legitimate response to neglect or unfavorable treatment. Neither do they show signs of nonviolent political militancy at the polls; while PAN's support is growing, its program is virtually indistinguishable from that of PRI, and its voting strength is not drawn disproportionately from the lower class. Indeed, the political sophistication of the lower class is shown to be so low that a recent attempt to hold primary elections for PRI candidates ended in mass confusion. Rather than focusing exclusively on the trade-offs and compromises among members of the oligarchy, the study might have shed more light on the future of Mexican politics by analyzing the sources and supports of this docility, resignation, and political naiveté among the poor, and the mechanisms which reinforce their lack of political mobilization and thus reduce their ability to challenge the system as it presently operates. The author concludes that "in the final analysis, the viability of the present Mexican political system and its increasing democratization seem to be contingent on the possibility of the regime's reducing the system of political patronage without destroying itself" (p. 183). We suggest that in Ensenada, the system's viability is far more dependent on the continued passivity and low aspirations of the lower class, qualities that may prove fragile indeed in a community so exposed to the demonstration effect of rising militancy among the poor in the United States.

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Student Politics in Argentina: The University Reform and Its Effects, 1918–1964. By Richard J. Walter. (Basic Books, Inc., Publishers, 1968. Pp. xviii, 236. $7.50.)

This is the first volume of the Basic Books series on student politics, edited by Seymour Martin Lipset (subsequent volumes have been devoted to French, Indian, and Chilean students). It is quite fitting that the first book in the series be a history of student politics in the nation which gave birth to university reform.

Although essentially a history, written by a historian, primarily for historians, a number of questions of interest to political scientists are posed in the book's Introduction: (1) "What have been the ages and socio-economic backgrounds of student leaders?" (2) "What factors have made students politically influential?" (3) "Have there been connections between national political parties and university organizations?" (4) "How influential have been professors, national political leaders, and intellectuals on student thought and action?" (5) "What percentage of the university youth have participated in political activities?" (6) "Has the student movement been of one defined political tendency?" (7) "What has been the relationship between the quality of education and student political activity in national affairs?" (8) "What have been the political attitudes and activities of student leaders after graduation from the university?" (pp. x–xiv) These questions are given brief, partial, and often tentative, answers in the book's Conclusion; however, the seven chapters in between ignore these questions, while concentrating almost exclusively upon historical narrative.

The best parts of the book are probably those sections which describe student activities in 1918 and 1958, two periods of well coordinated political action directed toward the attainment of clearly articulated goals. In 1918, when both the national government and public opinion were basically sympathetic, the students gained acceptance of virtually all their demands for university reform: optional classroom attendance, no tuition, periodic review of professorships, university autonomy [extraterritoriality], and most importantly, student participation in university government. On the other hand, forty years later, faced with the determined opposition of the national government, and having alienated the general public to a great extent, the students' attempt to prevent the legalization of private universities was a total failure.

Also well documented is the political naïveté of Argentine students, especially with regard to their views of organized labor and the armed forces. For more than half a century now, the students have tried to effect an alliance with the workers, but with almost complete lack of success in the period covered by the book; the closest thing Argentina has had to a labor government—the administration of Juan Perón between 1946 and 1955—was vehemently opposed by the students who considered Perón a right-wing dictator. At the same time, the students have long felt that the armed forces should be "the arm of the people" which can be called upon to depose unpopular regimes, but which should never itself govern—a more than slightly unrealistic position in post-1930 Argentina. Walters refers to this as a "propensity to favor rapid action and change and the corollary failure to anticipate or consider long-term effects and consequences" (pp. 188–189).

Perhaps the most disappointing feature of the book is that it is so badly out of date. Although published in late 1968, the book ends its coverage in 1964; and in fact very little is reported of the 1959–1964 period. This is apparently because the field research for the dissertation from which the book is taken, was completed in 1964. This terminal date, however, is very unfortunate, for in July of 1966 (twenty-three months before the book's Preface was written), President Onganía intervened in all of Argentina's national universities, abrogating most of the principles of university reform. Complete control over higher education was transferred to the Education Secretariat; student representation in university government was eliminated; and all forms of political activity within the university were banned. Thus the Argentine university system of the last five years bears little resemblance to the system during the 1918–1964 period as described here.

Although this is certainly not a "how-to" book, it might profitably be read by both students and administrators in United States universities, especially if they would consult the works of Brignardello, Ciria, Inglese, and Silvert to bring the story up to date. University administrators might learn that student participation in university government, at least in the case of the first nation to try it, does not automatically lead to chaos, but rather that students are capable of contributing substantially to the modernization of higher education. At the same time, students in this country might well reflect upon the fate of their counterparts in Argentina—and in Brazil, Mexico, and Peru as well —for when the Argentine students resorted increasingly to violence, they lost almost all their earlier public support, and thus proved to be virtually impotent in the face of an openly hostile military government. The predictable result was the immediate loss of all their hard-won gains of the previous half century.

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The Changing Structure of Europe: Economic, Social, and Political Trends. By Robert H. Beck, Harold C. Deutsch, Philip M. Raup, Arnold M. Rose, and John G. Turnbull. (Minneapolis: University of Minnesota Press, 1970. Pp. 286. $9.50.)

The study of regional integration, in Europe and elsewhere, is in need of genuine interdisciplinary work. To date, the field has been largely dominated by political scientists and economists working separately with different paradigms. Political scientists have been primarily concerned with identifying, measuring, and theorizing about the relationships among different dimensions of integration—focusing on political institutions, elite and mass attitudes and interactions, as well as a variety of social and economic transactions. Economists have been less concerned with models of integration than with theorizing about its consequences for international trade and total global welfare, economic growth within the integrating unit, and even with its impact on business practice and life style. Somewhat to my surprise, economists have done relatively less empirical work than political scientists—one notable exception being Lawrence Krause's *European Economic Integration and the United States*. Only Amitai Etzioni, among sociologists, has shown a significant concern with integration.

This lack of interdisciplinary work is always something of a tragedy for the social sciences. Whether we are interested in public policy or in social processes, disciplinary boundaries virtually always limit our vision. At best, these boundaries are an analytic convenience which justifies a partial paradigm. At worst, they are historical curiosities which encourage and excuse our limited vision. In the field of integration studies, the problems are compounded since it is scarcely possible to study integration within the confines of a single discipline nor, in my judgment, can the component parts be satisfactorily extricated from one another. Accordingly, scholars must either learn the very difficult skill of transcending their disciplines, or we must find ways to pool our concerns so that we can begin to pull together.

For these reasons, it was with considerable anticipation that I picked up this volume on *The Changing Structure of Europe* by a "research team" from the University of Minnesota, composed of an historian, Harold C. Deutsch; a sociologist, the late Arnold M. Rose; an economist, John G. Turnbull (assisted by Jean Belden Taber); an agricultural economist, Philip M. Raup; and an educational philosopher, Robert H. Beck. So much about the project seemed promising. It had apparently been carefully nurtured in faculty seminars between 1963 and 1966. The field work included ample time in Western Europe and extended to a summer in Eastern Europe for three members of the team. The authors chose to range broadly across trends rather than to zero in on a narrow and precise dependent variable. I began to read with the hope that some new ground might be broken and that we would, at the very least, begin to see the rewards beckoning to those who were willing to press ahead with interdisciplinary work.

Perhaps my expectations were too high, particularly in light of the rather modest claims made by the authors themselves:

> This is intended to be a summary volume on the research of the five contributing scholars, and it is addressed primarily to the interested reader, not the specialist (pp. 8–9).

Moreover, since *individual* detailed monographs by each of the scholars are supposed to follow this collection, perhaps interdisciplinary integration was not the overriding concern of those involved in the project. In any case, the book, as it turns out, is a rather uneven group of essays. If each were as compelling as the work of agricultural economist, Philip M. Raup and sociologist, Arnold M. Rose, then the book could be highly recommended—with or without a real interdisciplinary payoff. But the other essays tend to be rather cursory, although economist John G. Turnbull does bring together a good deal of information in an altogether satisfactory manner. Moreover, I must question the theme that holds the volume together: the premise that integration is largely a non-zero sum game from which the entire West—whether considered by region, by nation, or by social class—will benefit in the short as well as the long run. The authors all, in other words, lean rather heavily on "end of ideology" pluralist assumptions. I consider this unifying theme unfortunate because it is unquestioned and largely implicit. The result is that almost of all of the participants seem to think of integration as all benefits and no burdens. Certainly, there is little in the way of a sustained effort to make a balanced appraisal of burdens and benefits.

Philip M. Raup's study of the "constraints and potential in agriculture" is, on the other hand, a first-rate work of political economy—sensitive to social, political, and economic variables. Raup traces the way in which Common Market policy makers have been constrained by cultural traditions and the resultant structural rigidities to choose an agricultural policy which will be increasingly costly within the member states. He then shows that this process is likely to lead directly to a crisis of authority at the regional level, and that it is pregnant with potential conflicts with the rest of the world in general and the United States in particular. Professor Raup's brief essay is clearly the best analysis I have seen of the subject.

Arnold M. Rose's inquiry into cross-national migration is less far-ranging in scope but more ambitious in its effort to test hypotheses and uncover findings. Its essential conclusion is that, despite national differences, the

> cultural and political background of the migrants—by country, not as individuals—has little or nothing to do with the kind of adjustment they make in the immigrant society . . . what is important for integration and adjustment is the openness of the *immigrant* country—certainly in its overt policies, programs, and practices and probably in its informal attitudes as well (p. 217).

This finding must be taken as tentative given the rather simple conceptualization of certain variables and the questionable methods used to quantify them, but despite this reservation the essay remains a useful, systematic, and challenging beginning. Professor Rose soberly helps us to keep his cautiously optimistic findings in perspective by suggesting that, measured by the American experience of the pre-1921 period, "The Integration of Peoples" in Europe has been limited. "But—even at the height of the agitation of racist hatred—there was almost no one who insisted that the migrants return to their homelands after a period of labor in the United States" (p. 220). Of course, the American experience suggests that immigration may either be more or less than integration and, therefore, leads us to some more fundamental questions about the premises of Professor Rose's essay.

Unhappily, this volume tends to fall between stools. As an introduction to European integration it is not as systematic as it might be in covering the terrain and presenting findings. Professor Rose's essay, for example, does not—nor was it intended to—assess the growth of a transnational society, generally; it deals only with migration. Moreover, while some of the essays are professional in tone and make original contributions, others are cursory. Most important of all, the interdisciplinary promise of the book is not really fulfilled.

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The Soviet Union and the Law of the Sea. By William E. Butler. (Baltimore and London: The Johns Hopkins Press, 1971. Pp. 245. $12.50.)

Professor Butler of the University of London has given us a book that is topical in two respects. In the first place, he describes an evolution of Soviet theoretical views and diplomatic practice in an important area of international law and international relations, following the change in the status of the Soviet Union as a great power. In the second place, he analyzes the Soviet position in the light of the development of the rules of the law of the sea, which is an area of vigorous change and reform, resulting from technical progress and from the hunger for minerals and raw materials.

In spite of its revolutionary posture, the Soviet government has generally clung to the position of the imperial regime and has adhered to the accepted standards of international conduct. Some of the Soviet government's behavior was voluntary, dictated by the best interests of the country; some of it was enforced by the other powers (namely, Great Britain). This period of conformity and continuation, which according to Professor Butler lasted until 1960 (though some change in Soviet attitude came to the surface in 1946), occurred because the Soviet Union had no navy or merchant marine to speak of, and therefore had to meet other competitors on their own terms.

Since 1946, Professor Butler points out, the Soviet government insisted on increased strictness in enforcing its internal legislation regarding fishery rights. At the same time, theoretical writings in the Soviet Union acquired a new tone and formulated new aggressive demands. The extension of the concept of territorial waters, as well as the revival of such notions as "historical" and "closed" seas, and the reinterpretation of the freedom of the seas—all these were vigorously promoted and defended, while the institution of "innocent passage" was attacked.

The new tone of the Soviet legal literature was the result of two forces: Increased security concerns, and expansion of the Soviet navy spurred Soviet writers to criticize the traditional rules of international law as a basis of the claims of other maritime powers. And yet, Soviet literature must not be read as an interpretation of Soviet diplomatic policy. As Professor Butler puts it: "... Soviet international legal doctrine is often more important for insights into contemporary policy trends or preferences, than as a reliable guide to modern state practice" (p. 143). I have found this observation to be true in other areas of Soviet international relations as well.

A subtle, but demonstrable, change in Soviet sea policy occurred when the Soviet Union became a major sea power. As its fishing fleet came be perhaps the most sophisticated in the world, the Soviet government turned its attention to another direction. Insistence on security interests became muted. Freedom of the seas and navigation took precedence over the exclusive control of coastal waters, the doctrine of innocent passage was returned to favor, and closed sea doctrine was de-emphasized.

From a general point of view, Professor Butler argues very convincingly that there is no connection between Marxism-Leninism and Soviet approach to substantive issues of the law of the sea—an observation which is true of all other areas of international law. Thus, Professor Butler concludes that Soviet collaboration with other powers on the law of the sea not only has occurred but can reasonably be expected to continue.

The book is organized by subject rather than by periods. In seven major chapters the author deals with such topics as: (a) legal regime of territorial waters; (b) the regime of internal sea waters; (c) closed sea doctrine; (d) the continental shelf; and (e) the deep sea bed. The chapters on the continental shelf and the deep sea bed are of particular interest in view of Soviet innovations (in the Finnish case) in attempting to solve the delimitation problem and the still unresolved deep sea bed problem.

The book ends with a chapter containing a broad analysis of Soviet doctrine and practice; maps, tables of statutes and treaties, bibliography and an excellent index further commend the book to the attention of the reader.

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The Growth of World Law. By Percy E. Corbett. (Princeton: Princeton University Press, 1971. Pp. xii, 216. $7.50.)

The Structure of Impartiality: Examining the Riddle of One Law in a Fragmented World. By Thomas M. Franck. (New York: The Macmillan Company, 1968. pp. 344. $8.95.)

With the end of World War II the nations resumed their customary roles as makers of international law and simultaneous guardians of the peace and of sovereignty. As Wolfgang Friedmann clearly elucidated in *The Changing Structure of International Law* (1964), there was an acceleration of lawmaking that was a reflection of and response to the increasing number and variety of contacts between states that were fostered by public and private international organizations, science, technology, business methods, and a concern for underprivileged peoples.

Once again, lawmaking does not seem to have made the nations effective guardians of the peace, unless one chooses to treat as a major accomplishment the containment of violent conflict within the bounds of limited war and conventional weapons. Many are uncomfortable that twenty-six years of violence thus contained, slightly longer than the interval between the outbreaks of the two World Wars, is no indicator that guardians of sovereignty can also be guardians of peace.

Among the many who doubt the portent of accelerated lawmaking in a world still structurally decentralized are the authors of the two books under review. This is not to say that either author underrates the significance of international lawmaking. As Percy E. Corbett says at the outset of his book, law is essential to the organization, operation, and progressive development of community. But he regards it as no substitute for the common interest without which no community can come into being. No one who employs the state as his model of a community—and most Western scholars do so despite crime in the streets and other manifestations of disorder—is likely to disagree with Corbett's judgment that the level of common interest among nations hardly corresponds to the aspirations expressed in the ancient concept of a world community governed by natural law.

In concise prose, Corbett reviews progress toward "world order" in the sense of a supranational community that would not necessarily be a community of states. Nor, presumably, need world order be a philosopher's millennium that, as Corbett says, sustains man's faith in his potentialities. Insofar as there has been progress toward world order, it has been in the form of half measures taken in the effort to control forces endangering mankind. These successive half measures provide the substance of Corbett's review of the development of public international law, progress in world and regional organization, the relationship between law and war in the light of Vietnam, and the problem of controlling international propaganda.

Corbett occasionally suggests things that might be done to improve international law. For example, to avoid the farce of an invitation to another state to intervene on behalf of a political faction as in Czechoslovakia and Vietnam, Corbett suggests a convention requiring collective procedures that would be both a first step toward badly needed enforcement by defining just what is to be enforced and an educational device. Indeed, a sustained campaign of popular education is urged as essential in order to counteract the myth that the state, governed by men who reject the evidence that it is an anachronism, is the best provider of peace, security, and welfare.

It is rather commonplace to assert that evolution toward a world order significantly different from the present international system requires, among other things, that international law as *ius inter gentes* make room for world law as a law for individuals, corporations, and other private organizations. Students of international politics concentrate so heavily on governments that they produce little to parallel the lawyers' study of the "new law merchant." That law is evolving from twentieth-century commercial arbitration, particularly by the International Chamber of Commerce Court of Arbitration at Paris; it is evolving even though its body of precedent is limited because records are made public in only those of the more than 2000 disputes since 1924 that have gone to formal adjudication. Corbett's section on commercial arbitration is nicely complemented by Franck's plea for the denationalization of disputes between private persons that are needlessly magnified when transformed into disputes between states merely to render them justiciable. Worse, keeping private persons out of international courts impedes the growth of a large body of gap-filling precedent.

Third-party procedures are the specific focus of Thomas M. Franck's *The Structure of Impartiality,* a work marked by its literary elegance. Franck also gives attention to the "power syndrome" (one-party imposition of its will by force) and the "compromise syndrome" (two-party lawmaking). But it is the "impartiality syndrome" (third-party lawmaking) that Franck examines with the thought that "the failure of the international community to develop a system of third-party lawmaking comparable to that of the national community may well prove to be the fatal error of our civilization" (p. 46). Aware that Western, but not Eastern, religions entail a constant judgment of man by God and of God by man and have helped to produce a society dedicated to judgment, Franck seeks to utilize the habit of judging by applying it more broadly to the disputes of nations under circumstances that ensure that impartiality is both present and visible to the litigants.

Franck's exposition rests on the hypothesis that the impartiality of third-party decision-makers is directly related to the extent to which the community utilizes third-party procedures. Franck treats the impartiality of a judge as the ability to perceive and work with hypotheses that have no provable validity, but are a shorthand for group communication of group values. Such treatment helps to illuminate the problem of the international judge who seeks commitments from societies that operate within different systems of legal hypothesis. Franck then examines such matters as the need for equality of impartiality, the conventional wisdom of such barriers to greater use of third-party procedures as reliance on nuclear deterrence, gaps in international law, and the psychological aspects of impartiality.

However, Franck's attention to the psychology of third-party decision making rests chiefly on older psychological studies and concepts. One's appreciation for his attention to the psychological dimension is thereby tempered with the wish that the wealth of more recent studies and experiments had been taken into account. One could also wish that both he and Corbett had considered whether third-party procedures at the international level are at about the same stage of development and utilization as those within nations at a time when the power to raise money and armies lay with local lords rather than with central governments. Doing so would seem to be a proper route to understanding the international legal system if one's model or reference point is to be the national legal order.

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The Limits of Coercive Diplomacy: Laos, Cuba, Vietnam. By Alexander L. George, David K. Hall, and William R. Simons. (Boston: Little, Brown and Company, 1971. Pp. 268. $3.50.)

In order "to formulate a theory regarding the uses and limitations of coercive diplomacy," George, Hall, and Simons have undertaken a systematic analysis of three recent crises in United States foreign policy: the Laos crisis of 1960–61, the Cuban missile crisis of 1962, and the Vietnam intervention of 1964–65. George supplied the introduction, the theoretical framework, and the analytical conclusion, as well as the chapter on Cuba; Hall contributed a discussion of the Laotian crisis, and Simons, an analysis of the decisions to commit ground forces in Vietnam. The three chosen instances of the use of coercive diplomacy and the events surrounding them are described as they appeared at the time to American decision makers, and the narrative is documented from open, mostly press, sources. The accounts are substantial, informed, and reliable; and even though, as accounts of policy making, two of the three—the Laotian and the Vietnamese—have been superseded by the Pentagon Papers which appeared in June-July 1971, the main line of the argument does not appear to have been significantly affected.

The authors view coercive diplomacy as one of several alternative strategies for employing force in international relations, and one that is distinguishable from three other types of controlled use of force: the quick decisive military strategy (the "'blitzkrieg," Hungary 1956); the strategy of attrition (as in guerrilla warfare); and the test of capabilities within very restrictive ground rules (the Berlin blockade). By contrast with these, in coercive diplomacy force is used in an exemplary, demonstrative manner, in discrete and controlled increments, to affect the opponent's will rather than his capabilities. Much of it consists of the use of threats (that is also blackmail), but "if threats alone do not suffice and force is actually used, it is employed in a more limited, selective manner" (p. 18). The two basic variants of coercive diplomacy are the "try and see approach, the weak variant, and the tacit ultimatum approach, the strong variant" (p. 27).

The principal finding of this series of studies is that this strategy, in spite of its attractions (above all, its economy) is viable only when a special set of eight conditions is present (including, for example, the strength of American motivation and the urgency and clarity of American objectives, the adequacy of domestic support, or the availability of usable military options). All eight conditions were present in the Laotian and Cuban cases but most were missing in Vietnam, and it is the absence of these contextual conditions that, in the authors' analysis, accounts for the failure of the intervention.

Even if the conditions are favorable, however, large problems of operationalizing the strategy remain. These include the risks inherent in each resort to an ultimatum, the problem of timing strong coercive threats, and so on. "For all these reasons," concludes George, the principal analyst,

> there will be few crises in which coercive diplomacy—and particularly the strong variant of it . . . will constitute a feasible and useful strategy . . . Military power is of greater value to diplomacy when it is not used too often. When it is used, it should not be relied upon to accomplish difficult and complex objectives. . . . The wise statesman of a powerful country . . . would be wise to threaten only on behalf of the most important of his country's interests and to make modest, not far-reaching demands on others. It is prudent, as well as moral that the strong should respect the vital interests of the weak" (pp. 250–252).

On this cautionary note the book ends, and it is difficult to dissent from its prudent and circumspect judgments. These judgments carry additional weight because the three case studies, in addition to furnishing theoretical distinctions, also facilitate an assessment of the policies of "controlled, flexible response" inaugurated in 1961 as a substitute for the strategy of "massive retaliation" of the Eisenhower era. The Kennedy administration's search for a "more diversified set of military options along the entire spectrum of violence..." was expected to open the way for "many more discriminating and effective uses of limited force on behalf of a variety of foreign policy objectives" (p. 7). The analyst aptly comments that "this expectation has not been fully realized" and proceeds to analyze the reasons, including those for the "Vietnam tragedy" in terms close to the wisdom of classical diplomacy (see especially chapter 5).

There is, however, a flaw in this approach. Not unlike earlier strategic theorists (from whom he differs in other important respects), George views force primarily in an instrumental fashion, to be manipulated more or less skillfully, in greater or lesser accord with available contextual advantages. His theoretical analysis does not clarify the purposes for which American force should be used, and it understates the degree to which the employment of force alters and contaminates these purposes. It regards the national interest (as laid down by the executive branch, for the Congress is curiously absent) as an adequate definition of these purposes and is content to assert, in realist fashion, that coercion is justified in defense of vital interests.

But the matter cannot be laid to rest so easily. Theoretical analysis must take note that the worldwide responsibilities and the global military forces of the Great and Super-powers call for criteria of purpose that go beyond the national. Nor is it irrelevant or "utopian" to observe in a discussion of coercive diplomacy that in signing the Charter, all members of the

United Nations solemnly took on the obligation of refraining in their international relations, from the "threat or use of force" (Article 2, paragraph 4). What Great Power can afford to risk forfeiting its legitimacy and the moral basis of its position by habitually engaging in what could be, in fact, illegal practices, on the grounds of defending its interests?

The answer must be, of course, that (barring exceptional circumstances) force can safely be employed at the global level, if it is employed at all, for societal purposes alone. If the Great Powers control most of the world's military force, the threats to use such force, always dangerous, must be governed by purposes wider than national. This would mean that the means of coercion need to be tied to political structures of broader responsibility.

The three cases—Laos, Cuba, Vietnam—show little evidence of concern for such responsibility on the part of policy makers. This concern would have had to be shown not only in intergovernmental consultations or discussion in international organizations, but also in the form of a conscious design for alignment with major sectors of world opinion. The global system of Great Power authority, however, makes only rudimentary provision for world responsibility, and for this reason each crisis and each threat or use of force is a source of new danger.

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The Fulcrum of Asia: Relations Among China, India, Pakistan, and the U. S. S. R. By Bhabani Sen Gupta. (New York: Pegasus, 1970. Pp. xvi, 383. $8.95.)

China's World: The Foreign Policy of a Developing State. By J. D. Simmonds. (New York and London: Columbia University Press, 1970. Pp. ix, 260. $10.00.)

These two books and their authors share a number of similarities. Both writers are men who have close personal and professional connections with the areas about which they write. Both men worked on their books at universities with good research facilities. Both subscribe to the generally recognized thesis that foreign relations are strongly affected by domestic considerations. Both men are conscientious researchers and uninspired writers. Consequently, their books are generally useful but frequently soporific.

Beyond these similarities, the books differ in scope and in the nature of their virtues and deficiencies. Bhabani Sen Gupta's book contains 46 pages of notes with a total of 664 footnotes (some of them a full page long), no bibliography, no maps and a fair number of typographical errors. Sen Gupta, an experienced Indian journalist, makes extensive use of contemporary newspapers and magazines as well as secondary sources. Among his primary sources on China is *Peking Review*, an English-language weekly, which frequently carries long quotes from *Peking People's Daily* and the official Party periodical, *Red Flag*. The author is careful to cite dates and page numbers for *Peking Review* references, but at least three times in nine pages (footnotes 20, 23, and 42, on pages 152, 155, and 159) he cites *People's Daily* by date, but gives no indication whether the articles cited appear in the Hong Kong American Consulate's serial translations (*Selections from China Mainland Press; Extracts from China Mainland Magazines;* and *Current Background*) or not. Such information would be helpful to those who do not read Chinese.

Dealing as he does with four countries—India, Pakistan, China and the U.S.S.R.—Sen Gupta seems to have fallen into the error, common among specialists, of assuming that most readers will be as familiar with the geography of the areas under discussion as he himself is. I assume that this is why there are no end paper or other maps—an inconvenient omission for those of us interested in but not very familiar with the areas along China's southern and southwestern perimeter.

Sen Gupta (or his proofreaders) have handled the romanization of Chinese and other names rather well but not without some error. On pages 188, 196, and 303, for example; respectively: Chou Yank appears for Chou Yang; Kahmir for Kashmir; and Chin Chia-king for Chen Chia-kang. Such errors in themselves are minor but they are perhaps symptomatic of a carelessness that makes the reader uneasy.

In the preface of his book the author is refreshingly frank about the human frailties to be found among all individuals and governments involved. His first chapter sketches the background of the various national interests in this area and suggests the direction in which events seem to be moving. His second chapter details the arrival and growing presence of Soviet influence in South Asia. The third gives a fair account of the challenges from and *to* China rising out of the border disputes, Tibet, and the Soviet Union's support of India. The fourth and fifth chapters describe and analyze the events and consequences of confrontation, while the sixth and final chapter spells out some of the perceptual problems of the area's protagonists and would-be analysts.

Sen Gupta's *Fulcrum of Asia*, plodding

though it is, plows some new ground and turns up some ideas which—while perhaps not all brand new—deserve attention. The way in which nationalism in former colonial or semicolonial nations evolves into something mighty close to imperialism contributes to an understanding of the complexity of Chinese, Indian, and Pakistani claims in the border areas of the subcontinent. Sen Gupta examines the Soviet Union's growing involvement in the subcontinent and the ways in which pragmatism and national interest (especially where China is concerned) tend to outweigh or alter Russian ideological considerations. This is a useful tool in interpreting recent and current developments in this constantly troubled part of the world.

J. D. Simmonds, born in China and long a professional observer of that country, has kept both his book and his footnotes leaner than Sen Gupta's. In citing his sources, Simmonds usually refers his readers directly to the translations. He has limited his scope to one country —China, has avoided secondary sources, does include some maps, and has had better luck with proofreaders than Sen Gupta. However, *China's World* is no more readable than the *Fulcrum of Asia,* and may even be less of a contribution to the literature.

Part of Simmond's problem is that he is working over historical ground already pretty well covered by a number of other specialists. He has produced a chronological account, with some analysis, of China's foreign relations up to 1969. His expressed aim is to contribute to the methodology of foreign policy study by setting "a number of the essential elements" of China's foreign policy line in "a framework of a developing temporal and geographical world view, and all in the context of the domestic strategic dynamism" (p. 1). Unfortunately the work falls short of this ambitious goal.

Simmonds spends considerable time recording the Chinese Ministry of Foreign Affairs' vice-ministerial appointments and its changing allocation of responsibility for its various departments. All of this he tries to relate, with varying degrees of success, to the Chinese domestic scene.

This book might have been simpler and more useful if Simmonds had merely stated at the outset that he was interested in doing a chronology of China's foreign relations with some speculation on possibly significant causes for and results of change over the years. But such an approach, especially if contained in a proposal aimed at getting a year's study leave or funds for research, is probably less likely to be successful than one larded with terms like "domestic strategic dynamism" and intentions of developing a "new approach" to methodology.

This criticism is not meant to imply that *China's World* is without merit. Simmonds has given a reasonably concise review of China's foreign policy and some useful insight and grounds for speculation on the factors involved in that policy. He reminds the reader of the importance, especially in an agrarian and still relatively unindustrialized and uncosmopolitan nation like China, of domestic forces and frailties. He sketches clearly the influence on China's leadership of long years of guerrilla tactics and the relation of those tactics to foreign policy. His treatment of the United Front concept and its role in both domestic and international affairs is neatly handled. But his chapter on the "Intermediate Zone" left this reviewer with a feeling of puzzled boredom.

This unevenness of treatment, plus Simmond's occasional speculative reference, as on p. 76, to "untested Cultural Revolution sources" (which are also not footnoted or identified in any other way) detracts from an otherwise adequate sourcebook on Chinese foreign policy.

To sum up: For students of Chinese foreign policy J. D. Simmons had produced a useful chronology with some ideas worth considering; Bhabani Sen Gupta has produced a rather more thought-provoking book on an area and problems about which, to date, there has been too much provocation and too little intelligent thought.

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Old World, New Horizons: Britain, Europe, and the Atlantic Alliance. By Edward Heath. (Cambridge: Harvard University Press, 1970. Pp. 89. $4.95.)

The Atlantic Fantasy: The U.S., Nato, and Europe. By David Calleo. (Baltimore and London: The Johns Hopkins Press, 1970. Pp. 182. $7.00, cloth; $2.95, paper.)

Starting from different perspectives, with markedly different styles and emphases, these two books show substantial agreement concerning Atlantic international relations. Stylistically, Edward Heath is easily distinguishable from David Calleo. Political actor and political analyst have different conceptions of the proper use of language as a tool for persuasion and thought, argumentation and reasoning. On the one hand, with Heath, there is a careful tone of prudence and moderation, an emphasis on the incremental nature of proposed change. On the other, with Calleo, we see a higher emphasis on elegance in metaphor and analogy, on flamboyance and originality. Substantively, Heath is more concerned with the relations of Britain, the new Europe, and the wider world. Calleo, preoccupied with American over-extension in the world at large, deals incisively and at some length with the nature of the Cold War, with Atlanticism as an ideology, and with the more limited problems of NATO, monetary relations, Gaullist foreign policy, and the implications of European independence from the United States.

Beneath these and other differences, however, lies a relatively common understanding of the meaning of history, the nature of present international problems, and the profile of an appropriate strategy for the future. Both authors would move to restructure Atlantic relations by encouraging American withdrawal from Europe and simultaneously going forward with Western European integration. While increasing the military scope of Western European cooperation, they would leave the drawing together of all of Europe for a later day.

The political collapse of Europe after two world wars and the success of American nation building gradually led to massive American involvement in international affairs. Both authors believe that this involvement is excessive, and both would cut it back to an earlier and more modest scale, Heath recalls George Canning's "memorable words" to the House of Commons in 1826 and "reverses the old proposition." "I called the New World into existence to redress the balance of the Old," said Canning. For Heath, "the Old World must now be brought together to redress the balance of the New" (p. 11). Calleo believes that overambitious American "aspirations and worldwide commitments" have led to an "American crisis." He says that "'future historians may be puzzled by the present-day rebirth of American isolationism—not because it came at all, but because it took so long in coming" (p. 3).

European cohesion may help to alleviate American burdens. Heath believes that it is now possible that some of the American "responsibility for . . . world security," some of America's "loneliness" can be shared. In this perspective; British "commitments" outside Europe should be maintained. Indeed Western Europe might "in time come to carry out these obligations or some of them jointly as a European mission" (p. 69). For Calleo a more cohesive Europe might shift "the primary burden for Europe's defense to the major Western European states," and "reduce America's external burdens without sacrificing her true interests" (p. 131).

Defense, for each, is one of the main dimensions of renewed effort for European integration. American withdrawal will be facilitated by the creation at last of a European defense system, based largely on existing French and British nuclear capabilities, as well as German conventional forces. Heath believes that "the British and French nuclear forces should be pooled to form a joint deterrent which would be held in trust for Europe" (p. 4), and Calleo agrees in his discussion of "the problems of organizing the European deterrent" (p. 127).

While Germany will continue to supply conventional forces, it will not have its own nuclear weapons. "The United States," Calleo says, "has no interest in a demilitarized West Germany," but "no one wants the Germans to have nuclear weapons of their own" (p. 127). Heath notes "a new determination to stand up for German interests in defense," but accepts the validity of German renunciation of "'national control and national ownership of nuclear weapons" (pp. 71, 83).

Such a system will hopefully help to forestall nascent German concern about full rearmament, in the absence of a substantial American presence, while continuing to maintain a strong front against the Soviet Union. "Without reasonable satisfaction from the allies," Calleo believes, "the Germans would inevitably be compelled toward either a separate nuclear force or a close accommodation with the Russians" (p. 128). Heath recognizes a "legitimate interest" of the Eastern bloc in "ensuring that Germany does not emerge as an independent military power dominating Central Europe" (p. 73).

Heath has a somewhat darker view of the Soviet Union, but both agree that wider European unification is a task for a more distant future. Only after the construction of the system described may one hope "for that even wider unity on which rest the hopes of those still enslaved" (p. 84), says Heath. For Calleo his strategy is the prerequisite to "thrust apart the superpowers from their prolonged and increasingly inane confrontation along the Iron Curtain" (p. 145).

This shared outlook leaves something to be desired. In terms of a window on the past, the experience of the last twenty years does not support the assumption that the best way to promote international cooperation is through common military tasks. NATO's problems, with which Calleo deals at length, illustrate this point; and it may well be that the European Communities made what progress they did because, rather than in spite, of the early failure of EDC.

As for the future, an alternative strategy might take the possibility of American withdrawal from Europe as a first step in reducing military capabilities from one end of Europe to the other, freeing scarce resources for domestic and international activities in other social sectors. Calleo looks in this direction to some extent when he states that "there is much to be said for an inspected nuclear free zone in Central Europe, including both Germanies, Poland, Czechoslovakia, and perhaps the rest of the Balkans" (pp. 129–130). This initiative, however, seems to bear more heavily on some Europeans than on others. Elsewhere, he implies the importance of nonmilitary activities for the super-powers." Perhaps they might even come to believe," he concludes, "that their greatest contribution to the world community lay in the civilization which they achieved at home, rather than the power they exerted abroad" (p. 145). This could just as easily be true for all participants.

Calleo's study is an incisive analysis of the pattern of Atlantic international politics. His emphasis on fantasy, myth, illusion, and ideology, and his hardheaded discussion of the disproportional costs and benefits of Atlantic relations provide a welcome addition to the existing literature. Heath's lectures are most interesting because of his position as British Prime Minister. While the material is drawn largely from standard Atlantic legends, the form which it assumes under the hand of a man at one center of Atlantic power gives it a particular importance.

In spite of their differences, the most significant aspect of the two works may be the substantial degree of policy convergence which their arguments convey. One wonders if such agreement is a relatively chance happening, or whether it reflects some deeper emerging Atlantic foreign policy consensus that, as the American military presence in Europe is finally drawn down, Western Europe, moving as if to fill a vacuum, will expand and coordinate its military capabilities, maintain the division and subordination of Germany, and come to accept the permanent partition of all of Europe. If this is the case, then one may wonder also about the marginal benefits of further exploring the proposition that good neighbors make good fences.

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The Logic of Images in International Relations. By Robert Jervis. (Princeton: Princeton University Press, 1970. Pp. 281. $8.30.)

How can a power induce a rival to comply with its wishes while avoiding the actuality, or even a serious risk, of armed conflict? The problem has many dimensions. Some of these have to do with the balance of power and attempts to improve it, for example, by increasing one's arms strength, or recruiting allies. The present book, however, is concerned with a different dimension of the problem, namely, exercising influence over the other's conduct by suitable communication strategies, usually treated under the heading of diplomacy or propaganda. Mr. Jervis, however, does not discuss either in breadth but focuses attention upon one fundamental component of diplomatic and propagandistic activities, the production of "images."

An "image," as Mr. Jervis uses the term, is that picture of the entire behavioral profile of the communicator which his communications cumulatively implant in the recipient's mind. These "communications" include not only verbal messages but also significant nonverbal acts that provide cues about the communicator's behavioral profile. Mr. Jervis' fundamental contention is that images can have a significant effect upon the recipient's conduct in and by themselves, and, moreover, that such effects are eminently worth pursuing, because the production of images avoids the substantial costs and risks inherent in "balance of power" strategies. Images, he says, are produced "'on the cheap."

Verbal and nonverbal communications, the author says, contribute to the generation of images in two fundamentally different ways: they may act (or be interpreted) either as "signals" or as "indices." "Signals," he explains, "are statements or actions the meanings of which are established by tacit or explicit understandings among the actors" (p. 18). To put it more sharply: Communication by signal presupposes a code common to the sender and the recipient; the information encoded by the sender will be the one the recipient will obtain by decoding the signal. Mr. Jervis notes, as a fundamental characteristic of signals, that they can be true or false, and what is more, sincerely meant or deliberately misleading, their text as it stands leaving it entirely open whether they are one or the other. Hence the decoded meaning of a signal can function as part of the sender's image only when it is weighted for truthfulness, an operation necessarily independent of decoding as such.

Communications as "indices" are a different matter. The information they convey in *this* capacity is not that encoded by the sender using a conventional verbal or nonverbal language, but that kind of information content that natural events carry, the decoding of which involves breaking the code of "nature." The index function of a signal, for example, may reside in behavioral symptoms such as uncontrolled voice inflections (or changes in the rate of heartbeat), that cast doubt upon the truthfulness of the signal. Information conveyed in the index mode will enter into the image as it is conveyed. It would seem, then, that senders have only very incomplete control over the images produced by their communications, since encoded signal information will be weighted by the recipient, whereas index information is supplied involuntarily.

The most fascinating part of Mr. Jervis's analysis of image production, however, is that devoted to casting doubt upon this conclusion. According to him, image producers have many ways of manipulating both the signal function and the index function of their communications. He discusses these techniques in considerable detail, drawing upon a vast body of literature covering many fields (history, psychology, strategic analysis, and so on).

As to the desirable images themselves, Mr. Jervis mainly stresses two behavioral profile elements as essential to impart: "resolution," the readiness to take high risks, on the one hand, and "peacefulness," lack of aggressive intent, on the other. This definition of the main objectives of communication strategy essentially goes back to the deterrence doctrine of the recent past and thus gives the whole analysis a somewhat dated air. This applies particularly to the discussion of some concrete case material, for example, the Cuban missile crisis. The treatment of the problem of disengagement in Vietnam, contained in the last chapter, however, also contains some remarkably original and apt points. Generally speaking, the study has the merit of providing a number of fresh insights into the way in which the self-projected image of the actors enters into the international interaction process.

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The Theory and Practice of Neutrality in the Twentieth Century. By Roderick Ogley. (New York: Barnes & Noble, Inc., 1970. Pp. 217. $8.00.)

It is a most useful endeavor to present the student of international affairs and of modern history with an up-to-date picture of the law and practice of neutrality, since this specific pattern of political behavior of governments and nations does generally not enjoy great sympathy with the medium and great powers, and certainly not in the Anglo Saxon world. It is, therefore, incompletely known. Mr. Ogley, a Lecturer at the University of Sussex, gives such a picture in a new volume of the well known World Studies Series edited by James Henderson. He proceeds by reproducing selected documents referring to his subject, with a substantial introduction of his own and short introductory remarks to most of the documents.

Documents? Along with such truly documentary materials as extracts from the Hague Convention of 1907, from the very interesting exchange of notes and declarations surrounding the neutralization of Belgium and its violation by Germany in 1914, from speeches and resolutions relative to the policy of neutrality of the USA in the opening phases of the two World Wars, and many similar texts, the author/editor reproduces a great many passages from historical writing and from articles in political journals. This creates a serious imbalance. Whereas documents in the true sense reflect the spirit of the moment and attitudes of the actors involved at the time of their writing and speaking, the historian's and the politician's writings represent a retrospective description of what has happened or what they believe happened or what they would like to happen. The value for the reader, then, depends entirely on the quality of each presentation, and on his ability to situate the writer correctly.

Among Ogley's somewhat haphazard selections from books and magazine articles, some provide a well balanced picture of particular situations of neutrality, such as Ambassador Hägglöf's brilliant notes on Sweden in World War II, or Emile Cammaerts' biography of King Albert I of the Belgians. Others are of altogether different quality: some partisan such as a book by E. Borchard and W. P. Lage on American neutrality (1940), others pleading a specific cause, such as the piece by Max Petitpierre, a former Foreign Minister of Switzerland, urging the world to believe that his thoroughly passive and timid foreign policy had gained a new dimension by simply adding to the traditional concept of neutrality the word "solidarity." To this category of special pleas belongs also the final section of the author's introduction to the book, where he concludes his otherwise objective assessment of legal, economic, strategic, and political factors of neutrality with a plea for a neutral Britain, or where he deals with a chimera such as "civilian defense" in contrast to active resistance to an aggressor.

Ogley groups his material historically, in five parts, beginning with the neutrality of Belgium, passing then to World War I, from there to the days of the League of Nations, to World War II, and finally to the atomic age. This classificatory method is not very helpful when it comes to show different types of neutrality—permanent neutrality, *ad hoc* neutrality, neutralism, nonbelligerence, nonalignment. The author/editor is most successful when he examines the nature of nonalignment as clearly distinct from neutrality; he describes nonalignment as an offshoot of the Cold War and a policy which does not prevent its authors from being violently partisan in other respects. On the other hand, he is vague when it comes to drawing the boundary between permanent neutrality and a neutral attitude in some particular conflict, or between neutrality and the mere fact of not being actively involved in war. It seems strange, for example, to describe the Soviet Union, bound to Germany by the treaty of 1939, waging war against Finland, as a neutral.

In his own parts of the text, the author gives an excellent account of the strategic conditions of neutrality, explaining how the skillful use of deterrence—using military and economic arguments—is a condition of survival of neutral nations, and what the preferred instruments of deterrence of small nations may be. He seems, however, to contradict himself when he asserts that a guarantee by a great power is the strongest military asset of a neutral, having said himself, in another context, that a guarantee by *one* power would be in contradiction with any policy of neutrality.

Such logical contradictions, along with a few material errors pointing to some lack of familiarity with the subject, and the too narrow choice of titles in the Selected Bibliography limit somewhat the usefulness of the book as a true introduction to an interesting and little known subject. Having pointed to this inherent weakness, the reviewer is glad to endorse fully the author's conclusion, when he writes:

> . . . neutrality, far from being an anachronism, is a condition that states are likely to find themselves in, by accident or design, with increasing frequency in the last third of this century. . . . The neutrality that we are likely to see will, then, be a somewhat messy neutrality; its rules may be improvised, and the powerful may be able to disregard them. But there will persist . . . a reluctance of third parties to involve themselves in other's conflicts, which may well lead to a dampening down of these conflicts . . . (p. 205).

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Small Nation Survival: Political Defense in Unequal Conflicts. By V. V. Šveics. (New York: Exposition Press, 1970. Pp. 271. $8.50.)

In recent years the literature of international relations, which traditionally has been about great-power politics, has been supplemented by a number of useful books about the behavior and role of small states in international politics. While David Vital has spelled out some of the consequences of the material inequality of states for unaligned small nations, Robert L. Rothstein has analyzed the security problems of small powers in alliances. Professor Šveics's concern is with unaligned small nations involved in conflicts with militarily superior powers. Drawing on some older writings about strategy, especially early nineteenth-century German works, and on more recent books about resistance movements (particularly those dealing with reactions to German occupation in the Second World War), he analyzes a variety of unequal conflicts, but notably that between Czechoslovakia and the Soviet Union in August 1968. He concludes that it is perfectly possible for a small nation successfully to defend itself against even a vastly superior military power, provided it knows how to do it and has willpower enough to go through with it.

The way to resist overwhelming force effectively, Professor Šveics argues, is to transfer the struggle from the military to the political level. If the small nation has been defeated militarily and has been occupied by the enemy, the defender should realize that the defeat is no more than an unimportant tactical setback and should continue the struggle politically by resorting to all possible means of noncooperation and resistance. Summoning up its full national strength, the small nation should engage the enemy where *he* is weakest, prevent him from gaining control of the *polis,* erode his grip on the state, and generally intensify the conflict until final victory is secured. Šveics does not claim that the idea of subordinating the military side of the conflict, or stage of the struggle, to the political is new. Indeed, he shows well how the relationship between war and politics was clarified more than one hundred and fifty years ago by A. H. D. von Bülow and Carl von Clausewitz; how the lessons they had drawn from the Revolutionary and Napoleonic Wars were almost forgotten in the nineteenth century; how they were taught again by T. E. Lawrence after his experiences with the Arabs; and how they have been put to use in the resistance movements and people's wars of recent decades. Šveics's forcible restatement of ideas which seem as relevant now as ever before is a worthwhile contribution to a subject in which old insights are sometimes forgotten in the search for new theories.

Nevertheless, it is clearly with action rather than ideas that Professor Šveics is ultimately concerned. Admitting his deep commitment to the cause of small states struggling to retain or regain national freedom, he sets out to explain the correct strategy for these states. With passion and determination he strives to convince his readers and to persuade them to act on his instructions. The book is essentially prescriptive—offering directions to intellectuals, administrators and others about their roles in resistance to the occupation power, making suggestions for Sweden, Switzerland, and other small unaligned states how best to prepare their defense, and so on. As a guide to resistance movement leaders, and as a warning to statesmen who contemplate subjugating a small nation by superior force, it is a persuasive piece of work, which for its effect relies to a considerable extent on the technique of repetition.

In the minds of the more skeptical readers this book may leave a few queries. Does the author, in his effort to put military power in its proper political perspective, perhaps tend to underestimate the importance of force in international politics? Is it not true, for example, that the survival of the smaller members of the old European states system was due to the balance of power operating among the great military powers at least as much as to the national political strength mustered by threatened small powers? Is he perhaps, in his endeavor to establish old truths about the efficacy of noncooperation and resistance, inclined to ignore improvements in some of the means available to modern governments bent on keeping down hostile nations? Finally, is his adherence to the creed of nationalism, in which he rightly recognizes the main source of resistance, too uncritical? Is it, for example, plausible to argue (as he does on page 134) that nationalism cannot be a source of aggression or an obstacle to international cooperation? Lurking in his distinction between the mature nationalisms of certain small states and the aggressive imperialisms of some great powers may be the notion of the intrinsic moral superiority of small nations, a notion which may be detected on other pages too, and which may be suspected of permeating the whole book. But it is essentially because of their relative powerlessness rather than because of any intrinsically superior virtue that small states tend to behave differently in international relations.

Professor Šveics's book shows again how different the international political process looks when viewed from the angle of the smaller members of international society; and it encourages one to hope that new insights may be provided by those scholars who examine the states system from levels lower than that of the great powers.

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The Reality of Foreign Aid. By Willard L. Thorp. (New York: Praeger Publishers, for the Council on Foreign Relations, 1971. Pp. 370. $12.50.)

The Reality of Foreign Aid is another in a steady succession of nuts-and-bolts books on foreign aid. These books carefully take apart the foreign aid process and examine it under the microscope, detail by tedious detail. This is not meant as a pejorative remark. The details of the planning, giving and receiving of foreign aid are important and need repeated examination to permit us to chart changed conditions and profit from past experience. But these details are not the stuff of glamorous, exciting books. Rather, books like *Th Reality of Foreign Aid* mute the glib sensationalism of many of the derogatory observations about foreign aid which enliven political rhetoric. They make the reader see that foreign aid is a tough, risky enterprise, bound to require compromises which are displeasing to all sides. For every accusation which seems to highlight glaring misconduct on the part of donors or recipients, there are commonplace explanations which indicate that every mishap is not necessarily somebody's fault, let alone the outcome of evil scheming.

Thorp's book proceeds, more methodically than most, from question to nagging question. Why are donor and recipient nations alike disenchanted with the foreign aid process? Why is it so difficult to bring about satisfactory progress in the underdeveloped two-thirds of the world? How does one go about transferring capital and expertise effectively? To which countries should aid go and for what purposes? How much should each receive and for how long? Should economic transfusions take the form of trade, aid, loans, or other means of transfer? There are also questions about the administrative and institutional aspects of aid giving: Should aid be more heavily private or public? Should it be given through bilateral arrangements or multilateral institutions? What coordinating mechanisms are needed within the recipient and donor countries and internationally?

For each of these questions, Thorp presents a careful analysis of the problem in historical perspective, giving full details of past experiences and of present social, economic, and political factors. He typically follows this analysis by outlining the advantages and disadvantages of various types of solutions and often by adding recommendations. He has included dozens of helpful tables to illustrate various aspects of the aid flow from the United States and other major donors.

Most of Thorp's recommendations are cautious and relate to minor reforms in aid giving rather than sweeping restyling of the aid process. Mr. Thorp, for five years the Chairman of the Development Assistance Committee of the Organization for Economic Cooperation and Development, is experienced enough in the difficult art of foreign aid administration to shy away from advocating dramatic reforms which excite academic planners but chill the souls of practitioners. Yet it may be just such bold changes which are required to transform the picture of foreign aid successes from one of modest upward trends to one of steeply rising trendlines.

A few randomly selected examples will suffice to indicate the tenor of Thorp's reform proposals: (1) Citizens of developing nations should ordinarily be trained in institutions established in their own countries or areas rather than in developed nations. (2) The burdens of loan repayments, which now strangle economic growth, must be eased through cooperative efforts of the lending countries. (3) Multilateral aid giving should be improved through better coordination of existing bodies, rather than through the creation of new agencies. (4) The United States should assume a larger share of the world's growing aid burden by making long-term commitments for economic development in the Third World.

The volume ends with an appendix on "The Foreign Aid Numbers Game" which debunks all types of foreign aid statistics. The reader is shown, as he has been throughout the book, that appearances are deceptive and that one needs a host of additional information to put every "fact" and statistic into proper perspective.

Though the book is scrupulously objective in its discussions of specific problems, it does not question the basic assumptions behind foreign aid. Thorp briefly examines some of the arguments in favor of foreign aid and, in many scattered comments, indicates that aid, measured by various economic yardsticks, is beneficial for donors and recipients alike. Yet the other side of the coin is hardly examined. We are not told in what respects foreign aid may be detrimental and whether the criteria by which benefits are gauged are adequate and fully defensible.

This reviewer shares Mr. Thorp's beliefs in the value of foreign aid to foster technologically advanced societies throughout the world. Nonetheless, it would be refreshing in a scholarly study to find reappraisal of basic assumptions along with analysis of goals and procedures. In the process of re-examination, an experienced analyst may discover truly new political approaches that may be the key to a more rapid and humanly satisfying development process.

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Designs in Diplomacy: Pages from European Diplomatic History in the Twentieth Century. By Mario Toscano. Translated and Edited by George A. Carbone. (Baltimore and London: The Johns Hopkins Press, 1970. Pp. 433. $16.50.)

This volume brings together eight separate articles and essays originally published by Professor Mario Toscano over a twenty-year period. They were revised and updated several times, the last in the process of being brought together for translation and publication in English. The final work of revision was done by two of Toscano's assistants in the spring and summer of 1968 when the author was already very ill, but he was able to review their work and give it his approval before his death.

Toscano's position as a distinguished diplomatic historian, as chairman of the commission supervising the publication of Italian diplomatic documents (for the period 1861–1943), as special advisor to the Italian Foreign Office in the postwar period with the rank of Ambassador, and as a sometime *éminence grise* behind certain foreign ministers of the 1950's, gave him an unparalleled opportunity to see the foreign policy process at work from perspectives not ordinarily available to scholars.

Despite the disparate issues and long time period covered in this collection, two themes stand out. At the forefront is the theme of failure. From the first essay on the "Failure of the Hungarian-Rumanian Rapprochement of 1920" to the last on the inadequacies of intelligence sources which led to failures of judgment, Toscano is preoccupied with agreements which were never consummated, approaches which fell flat, results which were disappointingly inadequate. Even when partial successes were achieved, as in the reestablishment of diplomatic relations with Russia and France in 1944 and 1945, the political benefits expected remained largely unrealized.

The second theme is Italian diplomacy and the Second World War. With the exception of the first essay mentioned above, all the others concentrate on different aspects of staying out of, getting into, and getting out of the war. These are not well-rounded syntheses of the whole story. They are a specialist's effort to bring to bear the evidence of newly acquired primary sources on problems which are already well known to his readers. Toscano is writing for a small, knowledgeable, audience of experts, and his essays assume a highly competent readership. In reading them one must remember that they are only part of the story, which is all Toscano intended them to be.

As I read the essays I drew a number of conclusions, some explicit, some implicit. One is that Benito Mussolini and Galeazzo Ciano were lightweights in diplomacy. I already believed that, but Toscano's work drives this conclusion home. The tragedy of Italy, as Toscano indicates, was to have such incompetents in charge. They had a first-rate, skilled professional as their Ambassador in Moscow from 1939 to 1941 but all his talent and expertise couldn't help Italy, given the deficiencies of his political superiors.

Another conclusion involves the character of the Italian-Russian *rapprochement* which the Germans prevented from coming to fruition in the spring of 1941. It was obviously based on the mutual recognition of spheres of influence between Russia and Italy, with Russia granted preeminence in the countries bordering the Black Sea and at the Straits, and Italy receiving similar recognition for the Mediterranean. Toscano remarks that the proposed understanding was strikingly similar to the Racconigi Agreements of 1909 between liberal Italy and Czarist Russia. This led me to reflect on the relevance or lack of relevance of ideology, Communist or Fascist, to foreign policy.

I was struck again by the importance of the "old boy" network. Emissaries are chosen, assignments are made, because someone is related to a *Marchesa* close to Salazar who can influence the Prime Minister to serve as an intermediary with the English. The efforts of the Italians to get out of the war on favorable terms in the spring of 1943 are directed to the British rather than the Americans because the Italian upper classes have contacts with the British aristocracy. It is not exclusively an aristocratic "old boy" network, however, but also a diplomatic one, from which the Soviet diplomats are not excluded. The negotiations for a *rapprochement* between Italy and Russia in 1941 were made easier because Ciano and the Soviet *Chargé d'affaires* in Rome, Helfand, had been junior foreign-service officers stationed in China together for a number of years. The record also indicates that in the last analysis these personal contacts and influences are not conclusive. None of these links was able to help Italy, with England or with Russia.

This is classic diplomatic history, drawn from the primary documents and the memoirs, written and oral. There is no indication that Toscano is aware of modern theorizing in the area of foreign-policy decision making. None of the models which have been developed—interaction models, organization theory, bureaucratic politics—are evident in this work. I can be accused of criticizing him unfairly, but in a conversation in the late 1950's he remarked to me that his work in the foreign office taught him the limitations of diplomatic documents. As presented here decisions are made by one or two people—Mussolini and Ciano, Hitler and Von Ribbentrop, Stalin and Molotov. Civil and military experts at best provide information, and occasionally an opinion. Mussolini, as a dictator, operating in a world of secrecy and conspiracy, overvalues intelligence information because his mentality gives more credence to data if it is obtained by secret means than by more straightforward routes. But this conspiracy view of history is not limited to dictators, nor is the "spontaneous and completely subjective" way in which Mussolini often made decisions. Perhaps Toscano knew what he was doing when, in spite of personal bureaucratic experience, he decided to follow his "old-fashioned" conceptual framework.

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China and the Foreign Powers: The Impact of and Reaction to Unequal Treaties. By William L. Tung. (Dobbs Ferry, N.Y.: Oceana Publications, Inc., 1970. Pp. xxii, 526. $15.00.)

As Arnold Toynbee observed in his introduction to the *Major Peace Treaties of Modern History, 1648–1966,* "The modern West has had many good things to give to the world in the field of science and technology, but in the field of international relations the effect of the Westernization of the world has been disastrous." To attest to the truthfulness of this statement, China is a case *par excellence.*

For thousands of years China had lived in its own world, the so-called "T'ien-hsia" (All-under-Heaven), and its relations with the peoples around its periphery—Koreans, Japanese, Vietnamese, Siamese, Burmese and others—were usually conducted in a Sinocentric system based on the assumption of Chinese superiority. This Chinese world order, generally speaking, was hierarchical in form, benevolent in spirit, and peaceful in operation. In Chinese terminology, it was called "wang-tao" (peaceful way), which was not only different from, but directly opposed to, the "pa-tao" (forcible way) or power politics of the West.

When Westerners came to China in the eighteenth and nineteenth centuries, they first demanded equality with China, and after dealing the Chinese several blows in war they imposed upon China a series of unequal treaties, which almost reduced China to a colony. According to Dr. Sun Yat-sen, the international status of China at his time was even worse than that of a colony. He called China a "hypo-colony" in order to indicate that she had many masters without anyone of them accepting the responsibility for her.

China's response to the challenge of the West was more than one century of turbulent changes, including the Self-strengthening Movement, the "Hundred-day" Reform, the "Double-ten" Revolution, and the May Fourth Movement. Because none of these changes seemed to be enough, mainland China finally went Communist in 1949—a total Westernization in the sense that Communism originated in the West.

This tragedy of modern China is the subject of William L. Tung's book, which contains twelve chapters, four appendices, and numerous notes. The first three chapters cover the historical background. Chapters 4 and 5 analyze the impact of the unequal treaties upon the sovereignty and territorial integrity of China. From Chapter 6 to Chapter 9, the author describes China's reaction to the unequal treaties and their final termination during World War II. In the last three chapters he discusses China's problems as a resurgent great power in Asia and beyond. He concludes the book with these words: "For the fulfillment of her national aspirations, all segments of China are prepared to seek international support wherever attainable, but will strongly react to any confrontations if unavoidable" (p. 410).

Dr. Tung should be congratulated for his excellent work in this book, particularly for its lucidity, brevity, and systematic arrangement. He has made good use of primary as well as secondary sources. It is especially gratifying to find a separate index of treaties at the end of the book. It is even more admirable for a Chinese writer to maintain such fairness and objectivity as are displayed in his treatment of so many controversial problems, including the Yalta Agreement (p. 271), the responsibility for the "loss" of mainland China to Communism (p. 299), and the international legal status of Taiwan (pp. 310–311).

The main thesis of this book is to prove that "although China's troubles were partly due to her own impotence, the Chinese attitude toward foreign powers in recent decades has had much to do with the national humiliation endured during the past century" (p. vii). To the extent that "to every action there is a reaction" (p. vii), the author may be regarded as successful. He could have done even better, however, if he had gone a little deeper and explained why America was tagged by the Chinese Communists as their number one enemy even though the United States was the most friendly to China among all the Western Powers, and why Nationalist China waived its war claims against Japan even though China suffered the most from Japan during the last hundred years. Here we need an insight into the ideology and political philosophy of the two rival regimes in China.

Another desirable improvement would be the addition of a few maps to those parts of the text where the problem of national boundaries is discussed. The simple map on the front page is not sufficient.

Some terms used by the author are neologisms with dubious value. For instance, to call the Nationalists' purge of Communists a "purification movement" (p. 236) is a translation from Chinese too literal to be comprehended by Western readers. The description of imperialists' penetration of China by means of railways as "railway imperialism" (p. 194) is also a peculiar coinage.

Finally, there are quite a few misspellings and printing errors. Of course, these and other flaws, which can readily be removed when the book comes up for revision, are minor impediments in a very useful book.

SAMUEL SHIH-TSAI CHEN
Central Connecticut State College

Bridge Across the Bosporus: The Foreign Policy of Turkey. By Ferenc A. Váli. (Baltimore and London: The Johns Hopkins Press, 1971. Pp. 399. $12.50.)

Bridge Across the Bosporus may be called a survey of recent Turkish foreign relations. It is the only book of that description to have appeared in any language in at least two decades. In fact, although there are some valuable specific studies, mostly by Turkish scholars, we would say there is not much literature of any kind on Turkey's foreign relations since World

War II. Prof. Vâli has consulted what there is and drawn much from Turkish newspaper reports and from personal interviews, and has finally given us the benefit of his long interest in Turkish affairs. (His several previous books and many articles in English are all on other subjects.) As an amply informative survey of a set of issues of which we have scanty knowledge, this book is likely to be very highly appreciated. There are apt to be differing views, of course, about its approach to foreign policy analysis and about some of its interpretations.

The introductory chapters present a general historical background, then observations on what the author calls the geopolitical, ethnic and ideological foundations of Turkish foreign policy, and then some description of public opinion. The four central chapters analyze relations with the parties at issue regarding the NATO alliance, the Straits, Cyprus, and the Arab-Israeli and other regional issues. Concluding the book is an evaluation of economic development and a final chapter bringing together the themes of the whole work.

The title, preface, and epigraph ("This bridge signifies Turkey's wish to join Europe." —Willy Brandt) might at first lead readers to expect the popularized, simplistic interpretation of Turkey as a Westernized or unambivalently Westernizing nation. The author does favor Westernizing tendencies, and he may be somewhat optimistic about them, but his treatment is certainly not simplistic. It seems to this reviewer almost always judicious and acute. He points out difficulties and costs: the development plan (including investment in the bridge) requires postponing some needed social benefits, NATO membership probably hinders Cyprus policy, ethnic solidarity might possibly come to endanger peace and security. There are tensions among ideals and many intricate choices involved in the "wish to join Europe," and the main body of the text is a sensitive guide to them.

Related to the major theme of Westernization and its problems are several minor themes that are traditional among students of international relations. They include the importance of geographic conditions, the quality of Turkish diplomacy, the interplay of indigenous and outside cultures, the sometimes drastic effects of Great-Power interactions and ambitions, the subtle relations between national sentiment and calculations of national interest; and also "the commotion created by the small but noisy exponents of red and black fanaticism" (p. 377). Except about Turkey's radical left and right and in a few unimportant summary comments (compare pp. xi with 68–70 and 35 with 210–218, for instance), a careful critical judgment is sustained throughout.

Turkey, of course, is no longer the close ally of the United States that it was from the late 1940's to the early 1960's. Chapter IV quickly reviews the origins and development of their "Cold War" relationship and then goes on to relate the incident which more than any other dissolved it. Trying to stop an imminent Turkish military intervention in Cyprus in 1964, President Johnson wrote Prime Minister Inönü that the U.S. and other NATO allies might not consider themselves obliged to protect Turkey if a Soviet intervention ensued. That part of Johnson's letter "gives me the impression," Inönü replied, "that there exists between us a wide divergence of views as to the nature and basic principles of the North Atlantic Alliance" (quoted from p. 131). The author remarks:

> Up to that time, the Turkish leaders had never seriously analyzed the context of American interests relating to Turkey or the Turkish interest as it related to the United States' role in the affairs of Turkey. That Turkey ceased to follow the American initiatives automatically, but acted after due consideration with regard to national interest, amounted to a change. It was, for instance, realized that all major countries of the Atlantic Alliance have, to some extent, mended their fences with the Soviet Union, whereas Turkey had stayed behind as the last inflexible "Cold Warrior." Similarly, Turkey had completely neglected her relations with the Third World. Consequently, she had found herself voted down on the Cyprus dispute in the United Nations by her official opponents (the Soviet bloc countries), by the neutralists, and also by some of her official allies. Thus the search for new orientations began.
>
> Within a few years Turkish diplomacy was able to bring the country from an uncomfortable isolation to a new complex of relationships . . . (p. 133).

This interpretation of Turkey's "Cold War" phase is not easily confirmed. Perhaps it is true that officials did not adequately analyze either American interests or the risks of a close alliance. Or possibly the Menderes government (1950–1960) did analyze them and decided that a sort of junior partnership best served its domestic interests, as it understood them, while at the same time complying with the requirements of the "free world community" as interpreted at that time by the senior partner and others. In other words the old policy might have been deliberately based on transnational and partisan domestic interests as well as national ones. Perhaps the author would agree, and add that maybe neglect of the national ones exacerbated the troubles of the later 1950s and helped produce the 1960 Revolution. Thus the interpretation might be more explicitly developed. In any case this book makes its best contribution in treating the new complex of relationships that grew up after 1964, and in setting them in long-range historical perspective.

Historical perspective informs just about everything in this book, and approximately half of it consists of the kind of history-writing exemplified in the *Surveys* of the Royal Institute of International Affairs. Such writing is short on explicit generalization. While manifestly a detailed narrative, it develops some coherent general themes, but leaves them and indeed leaves a whole complex analysis quite unexplicit. Consider the author's description of how Turkey and Greece have been "interlocked nations" (chapter VI). It ends with comments which do make the narrative themes plain enough but which do not form systematized generalizations. For instance: "They display hatred and contempt for each other, yet they are drawn together by self-interest, rivalry, or the admiration of what is different between them" (p. 269). How are they different from the *frères ennemis* that Raymond Aron described? The author seldom goes into what are now called theoretical questions. His comparisons of Turkish with Japanese, Iranian and Ottoman foreign policy are cogent but very brief, leaving the book's value for comparative analysis of a formal kind mostly to the reader's induction. But the volume contributes to political science in part because its kind of history-writing does. The analysis is not explicit, but it is there to be explicated.

Approximately another half of the book treats the most recent period directly. The several issues and the interests and views of different parties are thoughtfully described, in the manner of an experienced observer able to give both precise details and broad reflections. We simplify much by saying that he makes the new and still changing relationships intelligible most of all by showing us their meaning for Turks. For instance, Turks increasingly see foreign policy as tied in with issues of cultural development and national identity, not just with security and economic devlopment. Prof. Váli is sensitive to these interconnections. Perhaps the chief merit of his book for most American readers is its sympathetic yet judicious representation of the predominant (as well as some dissenting) views of Turks.

This is a thorough and perceptive monograph on Turkey's situation in the world. By lacing a survey with analytic insight and by elucidating Turkish views, it contributes to scholarly and enlightened public discussion.

CARL M. DIBBLE

Ithaca, New York

Revolution and Chinese Foreign Policy: Peking's Support for Wars of National Liberation. By Peter Van Ness. (Berkeley: University of California Press, 1970. Pp. 266. $6.50.)

There are pitfalls for Western observers who discuss Communist China's support for wars of national liberation abroad. One temptation would be to treat it mainly in terms of the Communist conspiracy and the Yellow Peril myth. The opposite tendency would be to approach the issue with a guilty conscience and an unqualified admiration for the new China. Fortunately, the book under review is a sober and objective study, free of such clichés and predispositions.

The stated purpose of the book is to analyze systematically both the theory and practice of Chinese support for revolutions in all countries of the third world (Asia, Africa, and Latin America). To begin with, Part I addresses itself to the Maoist ideology of the national liberation struggle. Included in the discussion are Peking's maximum and minimum programs for revolution as expounded by Lin Piao. The former, the ideal type, requires Communist leadership and is based on Mao's theory of new democracy, while the latter is construed broadly enough to include almost all vigorously pursued, anti-imperialist revolutions, regardless of leadership. Although the first three chapters of the book contain nothing new in either sources or interpretations, they admirably provide the necessary theoretical foundation of Chinese revolutionary policy for a careful inquiry into what the Chinese actually do in their dealings with foreign countries.

Undoubtedly the most significant portion of the book is Part II, which examines the actual practice of China's support for wars of national liberation immediately preceding the Cultural Revolution. Here the analysis is limited to a single year, 1965, and attention is focused on Chinese public endorsement of revolutions, relations with foreign revolutionary movements, and criteria for selecting countries as targets for revolution. From the findings of his empirical data, the author is able to conclude that national interest rather than ideological consideration has essentially determined China's international behavior. In 1965, for example, the primary factor influencing Peking's selection of targets for revolution was found to be the relative friendliness or hostility of foreign governments toward mainland China. Citing Burma and four other countries as illustrations, Professor Van Ness points out that countries governed by regimes friendly to Peking were not selected as targets for revolution—even when revolutionary movements were extremely active in those countries, and even though some of the movements were led by pro-Peking Communists.

The impact of the Cultural Revolution upon Chinese foreign policy is treated in the final section (Part III). A sharp shift is shown to have taken place in the pattern of Chinese support for wars of national liberation during the period of the Cultural Revolution and especially in 1967. Great emphasis was placed on replication of the Chinese revolutionary model and the propagation of Mao's thought, often to the detriment of local revolutionary efforts as well as Chinese foreign-policy interests. Nevertheless, the author correctly views this shift as only transitory and maintains that it reflected not so much the advent of a new foreign policy as the precedence of domestic concerns over international interests.

On the whole, *Revolution and Chinese Foreign Policy* is a well researched book. It presents some interesting information and perceptive comments. It contains ten useful tables and one helpful bibliographical note.

The main difficulty with the book lies in the narrowness of its scope. Although justified from the standpoint of research manageability, the confining of the study to a short span of three years (1965–1967) clearly restricts the value of its findings and generalizations. By the same token, the book suffers from another limitation in that its analysis is focused on Peking's *public* endorsement of foreign wars of national liberation without due attention to other forms of support.

Despite these shortcomings, the volume proves to be an important work and will serve as a valuable stimulus to further studies in this highly complex and fertile field.

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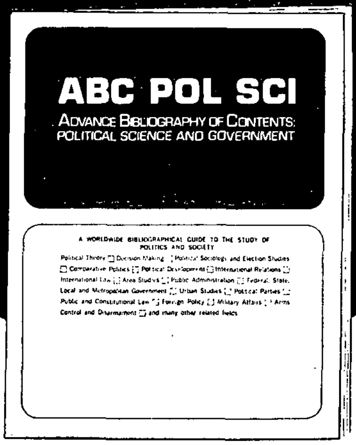

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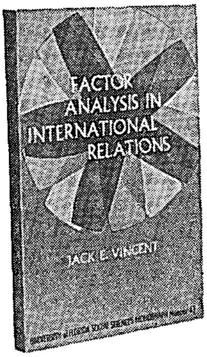

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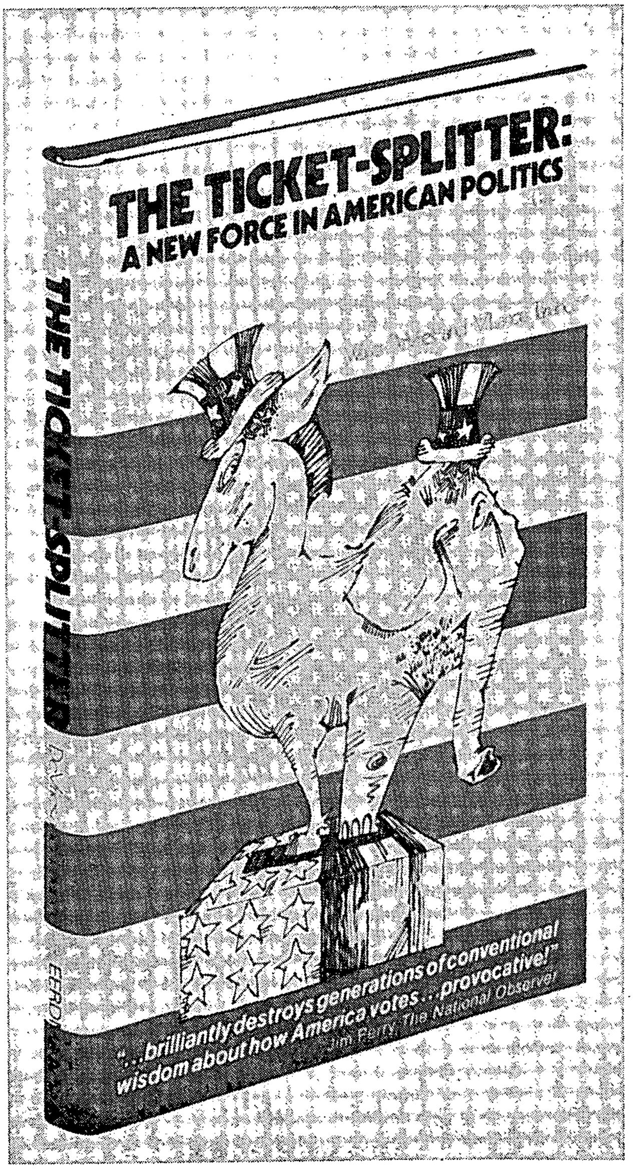
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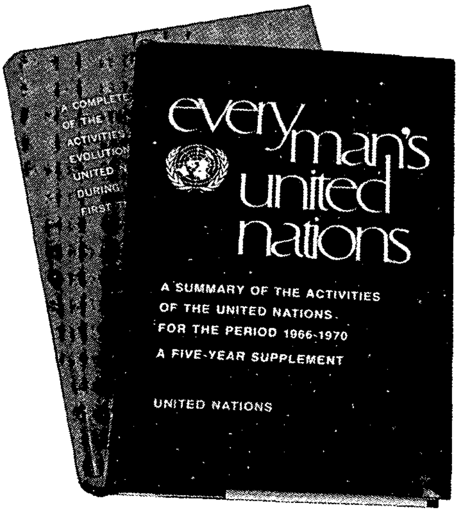
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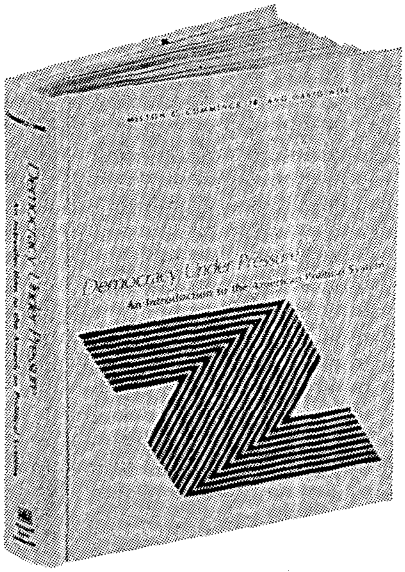

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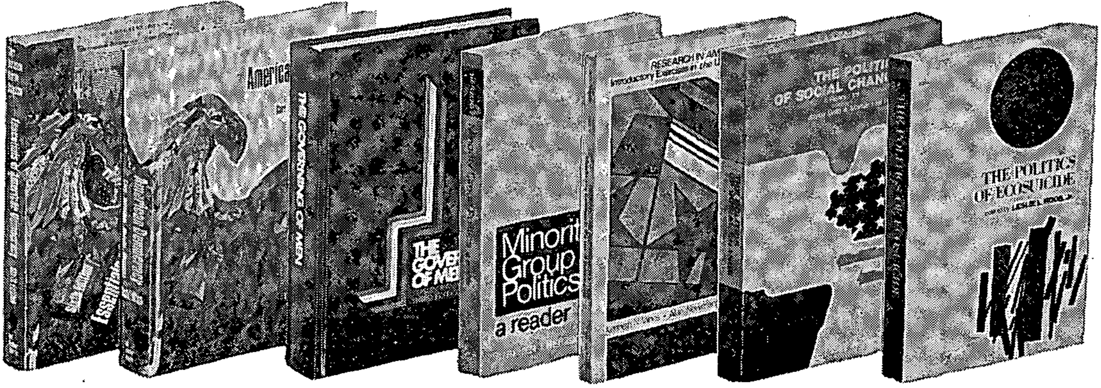
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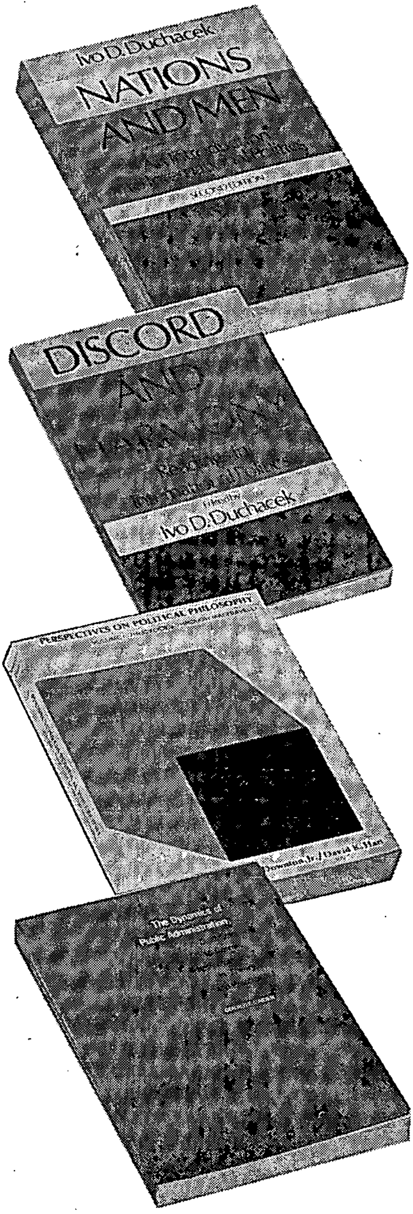
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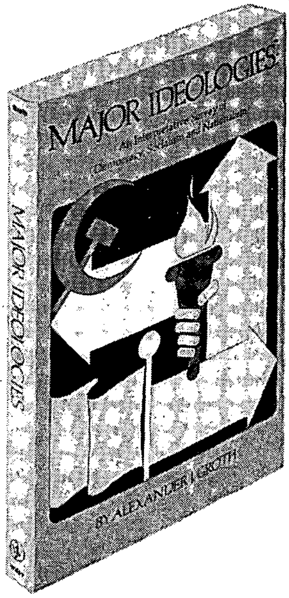
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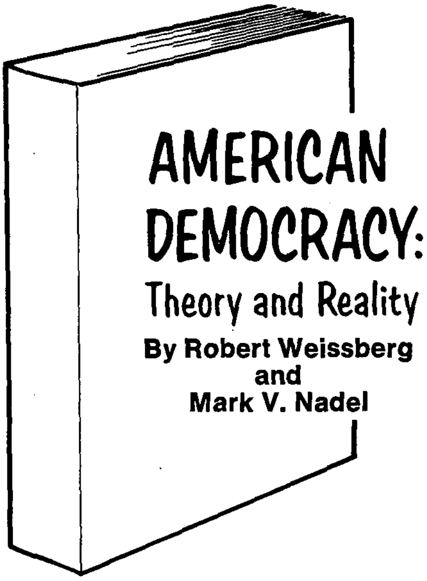
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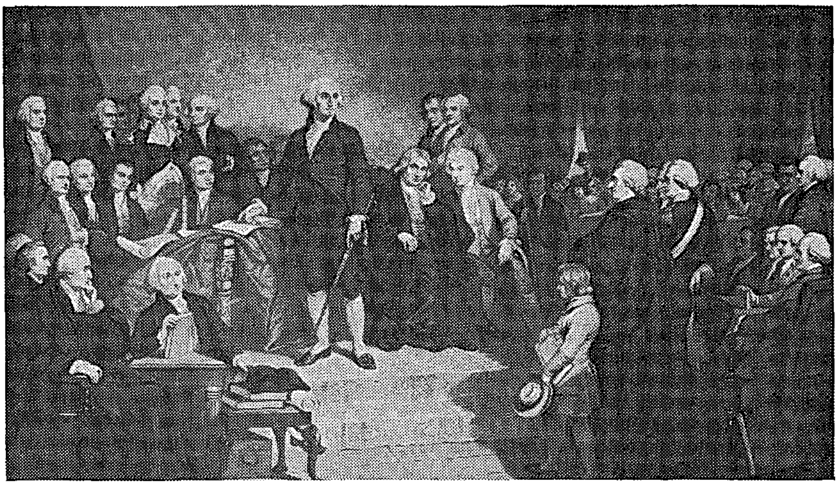

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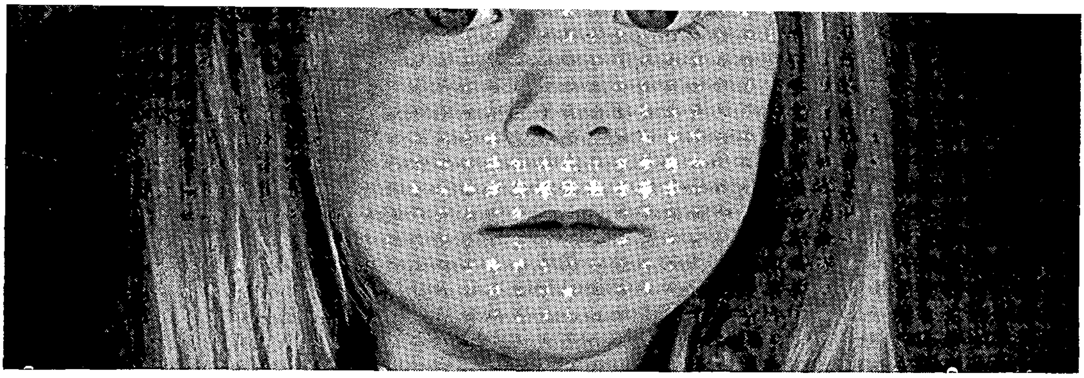

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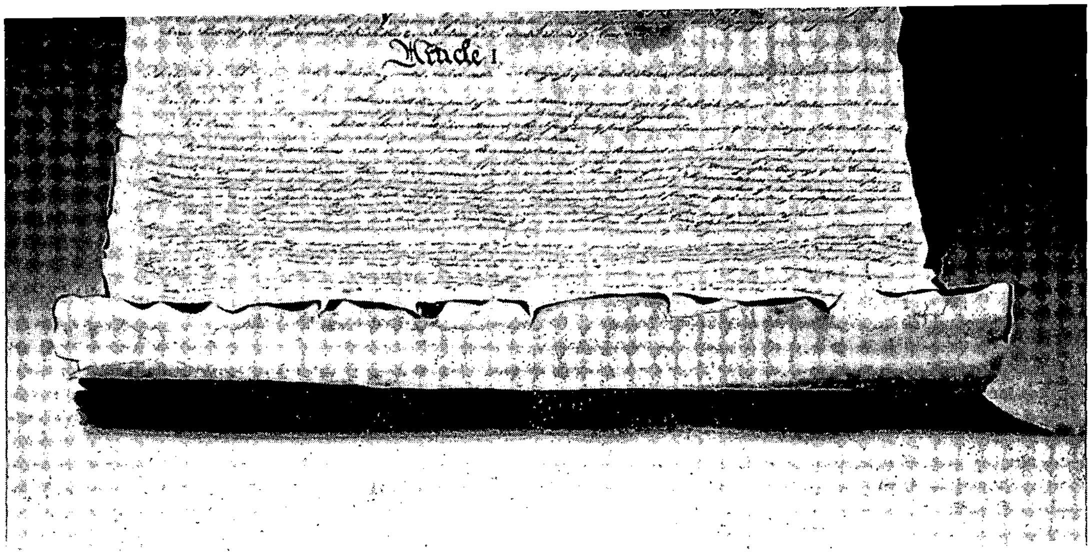

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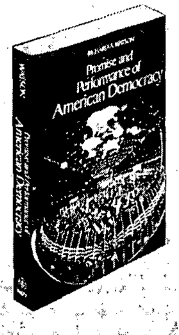

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American Elsevier Publishing 375
Arno Press 348
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Brookings Institution 304, 305
Univ. of California Press 362, 363
Cambridge University Press 388
Columbia University Press 313, 379
Consensus Publishing 312
Cornell University Press 392, 393
Dodd, Mead & Co. 338
Dorsey Press 327
William B. Eerdmans Publishing 367
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University of Florida Press 366
Free Press 330, 331, 332, 333, 334, 335
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Harper & Row Cover 2, 322, 386, 387
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McCutchan Publishing Corp. 389
McGraw-Hill Book Co. 360, 361
David McKay 314
Macmillan Co. 323, 324, 325
Charles E. Merrill Publishing 339
MIT Press 371
Northwestern University Press 402
Ohio State University Press 326
Oxford University Press 353, 354, 355
F. E. Peacock Publishers 352
Penguin Books 336
Politics 349
Praeger Publishers 306, 329
Princeton Information Technology 340
Princeton University Press 380, 381
Prentice-Hall 328, 341, 346
Rand McNally 368, 369
Random House 310, 311
Rutgers University Press 346
St. Martin's Press 308, 309
Scott, Foresman & Co. 385
Stanford University Press 399
State University of New York Press 365
Teachers College Press 364
TIAA 404
U. N. Publications 371
Van Nostrand Reinhold 359
University Press of Virginia 396
John Wiley & Sons 316, 317, 347, 382, 398
World Law Fund Cover 3
Yale University Press 328

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